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ARITHMETICAL PRODIGIES.

E. W. SCRIPTURE, PH. D. (Leipzig).

I.

A great deal has been said and written about these phenomenal persons in a very uncritical manner; on the one hand they are regarded as almost supernatural beings, while on the other hand no notice has been taken of them scientifically. Nevertheless, we can perhaps gain light on the normal processes of the human mind by a consideration of such exceptional cases. The first object of the present article is to give a short account of these persons themselves, and to furnish for the first time an approximately complete bibliography of the subject. Thereupon the attempt will be made to make such a psychological analysis of their powers as will help in the comprehension of them, and will perhaps furnish more than one hint to the practical instructor in arithmetic.

NIKOMACHOS.—Lucian said that he did not know how better to praise a reckoner than by saying that he reckoned like Nikomachos, of Gerasa.[1] Whether this refers to the reckoning powers of Nikomachos (about 100 A. D.), or to the famous Introduction to Arithmetic written by him, we are left in doubt. De Morgan inclines to the former opinion,[2] Cantor holds the latter.[3] The literal translation of the pas-

[1] Lucianus, Philopatris, "ἀριθμέεις ὡς Νικόμαχος."
[2] Smith's Dictionary of Greek and Roman Biography v. Nikomachos.
[3] Cantor, *Vorlesungen über Geschichte der Mathematik*, Leipzig, 1880, I, 363.

sage places Nikomachos undoubtedly among the skillful calculators.

AFRICAN SLAVE DEALERS.—Perhaps brought to the front or produced by the necessity of competing with English traders armed with pencil and paper, many of the old-time slave-dealers of Africa seemed to have been ready reckoners, and that, too, for a practical purpose,—a point overlooked by more than one of the later calculators. "It is astonishing with what facility the African brokers reckon up the exchange of European goods for slaves. One of these brokers has perhaps ten slaves to sell, and for each of these he demands ten different articles. He reduces them immediately by the head into bars, coppers, ounces, according to the medium of exchange that prevails in the part of the country in which he resides, and immediately strikes the balance."[1] The ship-captains are said to have complained that it became more and more difficult to make good bargains with such sharp arithmeticians. It was also an African who was the first to appear in this rôle in America.

TOM FULLER.—The first hand evidence in regard to Fuller consists of the following: A letter read before the Pennsylvania Society for the Abolition of Slavery by Dr. Rush of Philadelphia, which is published, more or less completely, in three places;[2] and the obituary which appeared in the Columbian Centinel.[3] On the foundation of these documents several later accounts have been given.[4]

[1] [T. Clarkson.] An Essay on the Slavery and Commerce of the Human Species, particularly the African. 2d Ed., London, 1788. (The passage quoted does not appear in the American editions, Phila., 1786, 1787, 1804).

[2] American Museum, Vol. V, 62, Phila., 1799.

Steadman, Narrative of a five years expedition against the Revolted Negroes of Surinam, South America, 2v. 4°, London, 1796, Vol. II, 260. In the French translation, Vol. III, 61.

Needles, Historical Memoir of the Penn. Society for the Abolition of Slavery; Phila., 1848, p. 32.

[3] Columbian Centinel of Boston, Dec. 29, 1790, No. 31 of Vol. XIV.

[4] For example, Grégoire; An Enquiry concerning the Intellectual and Moral Faculties, and Literature of Negroes, followed with an Account of the Life and Works of Fifteen Negroes and Mulattoes; Translated by D. B. Warden; Brooklyn, 1810. (The translation is from Grégoire's original manuscript.) Brissot de Warville; New Travels in the United States of America. performed in 1788; London, 1792, p. 287; 2d Ed., London, 1794, vol. I, 243; Boston, 1797 (reprint of 1st ed.), p. 158; in the original French edition, vol. II, p. 2. Williams; History of the Negro Race in America; New York, 1883, vol. I, 399. Didot's Nouvelle biographie générale v. Fuller.

Thomas Fuller, known as the Virginia Calculator, was stolen from his native Africa at the age of fourteen and sold to a planter. When he was about seventy years old, "two gentlemen, natives of Pennsylvania, viz., William Hartshorne and Samuel Coates, men of probity and respectable characters, having heard, in travelling through the neighborhood in which the slave lived, of his extraordinary powers in arithmetic, sent for him and had their curiosity sufficiently gratified by the answers which he gave to the following questions: First, Upon being asked how many seconds there were in a year and a half, he answered in about two minutes, 47,304,000. Second: On being asked how many seconds a man has lived who is 70 years, 17 days and 12 hours old, he answered in a minute and a half 2,210,500,800. One of the gentlemen who employed himself with his pen in making these calculations told him he was wrong, and that the sum was not so great as he had said—upon which the old man hastily replied: 'top, massa, you forget de leap year. On adding the amount of the seconds of the leap year the amount of the whole in both their sums agreed exactly."[1] Another question was asked and satisfactorily answered. Before two other gentlemen he gave the amount of nine figures multiplied by nine. He began his application to figures by counting ten and proceeded up to one hundred. He then proceeded to count the number of hairs in a cow's tail and the number of grains in a bushel of wheat. Warville says in 1788, "he has had no instruction of any kind, but he calculates with surprising facility."[2] In 1790 he died at the age of 80 years, having never learned to read or write, in spite of his extraordinary power of calculation.[3]

JEDEDIAH BUXTON.—Jedediah Buxton[4] was born in 1702, at Elmton, in Derbyshire, England, where he died in 1772.[5]

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[1] American Museum, V, 62.

[2] Warville, New Travels, p. 158.

[3] Columbian Centinel, loc. cit.

[4] Gentleman's Magazine, 1751, Vol. XXI, p. 61, 347; 1753, vol. XXIII, p. 557; 1754, vol. XXIV, p. 251, which are the original authorities. Chalmer's General Biogr. Dictionary, London, 1812, v. Buxton; Rose, New General Biogr. Dictionary, London, 1848, v. Buxton; Didot's Nouvelle biographie générale, v. Buxton; Michaud's Biographie universelle v. Buxton.

[5] The dates are given on the authority of Lyson's Magna Britannia, London, 1817, vol. V, Derbyshire, p. 157.

Although his father was schoolmaster of the parish and his grandfather had been the vicar, his education was by some chance so neglected that he was not able to scrawl his own name.[1] All his attainments were the result of his own pure industry; the only help he had was the learning of the multiplication table in his youth; "his mind was only stored with a few constants which facilitated his calculations; such as the number of minutes in a year, and of hair's-breadths in a mile."[2] He labored hard with his spade to support a family,[3] but seems to have shown not even usual intelligence in regard to ordinary matters of life. The testimony as to his arithmetical powers is given by two witnesses. George Saxe says: "I proposed to him the following random question: In a body whose three sides are 23,145,789 yards, 5,642,732 yards, and 54,965 yards, how many cubical ⅛ths of an inch? After once naming the several figures distinctly, one after another, in order to assure himself of the several dimensions and fix them in his mind, without more ado he fell to work amidst more than 100 of his fellow-laborers, and after leaving him about five hours, on some necessary concerns (in which time I calculated it with my pen) at my return, he told me he was ready: Upon which, taking out my pocket-book and pencil, to note down his answer, he asked which end I would begin at, for he would direct me either way. . . . I chose the regular method, . . . and in a line of twenty-eight figures, he made no hesitation nor the least mistake."[4] "He will stride over a piece of land or a field, and tell you the contents of it, almost as exact as if you measured it by the chain. . . . He measured in this manner the whole lordship of Elmton, of some thousand acres, . . . and brought the contents, not only in acres, roods and perches, but even in square inches; . . . for his own amusement he reduced them to square hairs-breadths,

[1] "His total want of education has been attributed to his excessive stupidity when a child, and an invincible unwillingness to learn anything." Lyson's Magna Britannia, V, 157, note.

[2] Journey Book of Engl., Derbyshire, p. 79.

[3] "A day-labourer," Lyson's Magna Britannia, loc. cit. "Either a small land-owner or a day-labourer; but probably the former," The Journey-Book of England, Derbyshire; London, 1841, p. 79.

[4] Gentleman's Magazine, XXI, 61.

computing (I think) 48 to each side of the inch."[1] Various other problems were solved by him with like facility on later occasions, before a different witness.[2]

From May 17 to June 16, 1725, he was (to use his own expression) drunk with reckoning, by which a kind of stupefaction was probably meant. The cause was the effort to answer the following question: In 202,680,000,360 cubic miles how many barley-corns, vetches, peas, wheat, oats, rye, beans, lintels, and how many hairs, each an inch long, would fill that space, reckoning 48 hairs in breadth to an inch on the flat? His table of measures, which he founded on experiment, used in answering this was:

200 Barley Corns,	
300 Wheat Corns,	
512 Rye Corns,	
180 Oats,	are contained in one solid inch.[3]
40 Peas,	
25 Beans,	
80 Vetches,	
100 Lintels,	
2304 Hairs 1 inch long.	

Quite curious is Buxton's notation for higher numbers. His system is: Units, thousands, millions, thousands of millions, millions of millions, thousand millions of millions, tribes, thousands of tribes, etc., to thousand millions of millions of tribes; cramps, thousands of cramps, etc., to thousand million of million of cramps; tribes of cramps, etc. to tribes of tribes of cramps.

In regard to subjects outside of arithmetic, his mind seemed to have retained fewer ideas than that of a boy ten years old. On his return from a sermon he never brought away one sentence, having been busied in dividing some time or some space into the smallest known parts. He visited London in 1754, and was tested by the Royal Society. On this visit he was taken to see King Richard III performed at Drury Lane playhouse, but his mind was employed as at church. During the dance he fixed his attention upon the number of steps; he attended to Mr. Garrick only to count the words that he

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[1] Gentleman's Magazine, XXI. 61.
[2] Gentleman's Magazine, XXIII, 557, XXIV, 251.
Gent. Mag., XXI, 348.

uttered.[1] At the conclusion of the play they asked him how he liked it. He replied "such an actor went in and out so many times and spoke so many words; another so many, etc."[2] He returned to his village and died poor and ignored.

AMPÈRE.—The first talent shown by André Marie Ampère,[3] *1775, at Lyon, †1836, at Marseilles, was for arithmetic. While still a child, knowing nothing of figures, he was seen to carry on long calculations by means of pebbles. To illustrate to what an extraordinary degree the love of calculation had seized upon the child, it is related that being deprived of his pebbles during a serious illness, he supplied their places with pieces of a biscuit which had been allowed him after three days strict diet.

As soon as he could read he devoured every book that fell into his hands. His father allowed him to follow his own inclination and contented himself with furnishing him the necessary books. History, travels, poetry, romances and philosophy interested him almost equally. His principal study was the encyclopedia in alphabetical order, in twenty volumes folio, each volume separately in its proper order. This colossal work was completely and deeply engraved on his mind. "His mysterious and wonderful memory, however, astonishes me a thousand times less than that force united to flexibility which enables the mind to assimilate without confusion, after reading in alphabetical order matter so astonishingly varied."[4] Half a century afterwards he would repeat with perfect accuracy long passages from the encyclopedia relating to blazonry, falconry, etc.

At the age of eleven years the child had conquered elementary mathematics and had studied the application of algebra to geometry. The parental library was not sufficient to

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[1] Gentleman's Magazine, XXIV, 251.

[2] Memoir of Zerah Colburn, p. 174.

[3] Bibliography (for his life), Saint-Beuve, M. Ampère, sa jeunesse, ses études diverses, etc., in the Revue des deux Mondes, 1837, 4 ième série, t. IX, p. 389. M. F. Arago, Eloge d' Ampère (given in a somewhat condensed form by E. Arago, in Michaud's Biogrgraphie universelle, nouv. éd., v. Ampère), translation in Smithsonian Reports, 1872, p. 111. Didot's Nouvelle biographie générale, v. Ampère. Valson, Vie d' Ampère, Lyon, 1886.

[4] Arago, Eulogy on Ampère, Smithsonian Reports, 1872, p. 113; Michaud's Biogr. universelle, I, p. 597.

supply him with further books, so his father took him to Lyon, where he was introduced to higher analysis. He learned of himself according to his fancy, and his thought gained in vigor and originality. Mathematics interested him above everything. At eighteen he studied the Mécanique analytique of Lagrange, nearly all of whose calculations he repeated; he said often that he knew at that time as much mathematics as he ever did.

In 1793 his father was butchered by the revolutionaries, and young Ampère was completely paralyzed by the blow. Rousseau's botanical letters and a chance glance at Horace roused him after more than a year from an almost complete idiocy; and he gave himself up with unrestrained zeal to the study of plants and the Augustan poets. At the age of twenty-one his heart suddenly opened to a new passion and then began the romantic story of his love, which is preserved in his *Amorum* and his letters.[1] Ampère became professor of mathematics, chemistry, writer on probabilities, poet, psychologist, metaphysician, member of the Academy of Sciences of Paris, discoverer of fundamental truths of electrodynamics, and a defender of the unity of structure in organized beings.[2]

Just as he began by learning completely the encyclopedia of the 18th century, he remained encyclopedic all his life, and his last labors were on a plan for a new encyclopedia.

GAUSS.—The arithmetical prodigies might be divided into two classes, the one-sided and the many-sided. The former would include those who like Buxton, Colburn and Dase were mere "reckoning-machines," the other would consist of men in whom the calculating power was only a part of gifts of mathematical talent like Safford, or even of the highest mathematical genius like Gauss.

Carl Friedrich Gauss was born in 1777, in Braunschweig. He was the offspring of a poor family that had in nowise distinguished themselves, although his mother seemed to have been of finer mental build than the paternal stock. Moreover his maternal uncle was a man of unusual talent: com-

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[1] André Ampère, Correspondence et souvenirs, Paris, 1873.
[2] List of Works in Michaud's Biogr. universelle, nouv. éd., I, p. 611.

pletely uninstructed he learned to produce the finest damask; in Gauss's opinion "a natural genius had been lost in him."[1] At an early age the genius of Gauss began to show itself. With the assistance of friends and of persons of the nobility he was enabled to get a school-education. At the age of eleven he entered the gymnasium where he mastered the classical languages with incredible rapidity. In mathematics also he distinguished himself. It is said that a new professor of mathematics handed back thirteen-year-old Gauss's first mathematical exercise with the remark that it was unnecessary for such a mathematician to attend his lessons in the future.[2] The Grand Duke, hearing of his talent, sent for him. The court was entertained by the calculations of the fourteen-year-old boy, but the duke recognized the genius and gave him his support. It is to be regretted that we have not fuller accounts of his early calulations, but his later achievements have so completely occupied the world of science that less attention has been paid to his calculating powers. It is curious to think that if he had had the misfortune to have been gifted with nothing else, he would probably have distinguished himself as Dase or Mondeux did; he might even have proclaimed himself in the Colburn fashion, as a miraculous exception from the rest of mankind; as it is, he was only the greatest mathematician of the century.

After leaving the gymnasium in 1795, he entered the University of Göttingen. As early as 1795, he discovered the method of the least squares, and in 1796 he invented the theory of the division of the circle.

In 1798 he promoted *in absentia* as Dr. phil. at the university of Helmstedt.[3]

In 1801, at the age of twenty-four, his *Disquisitiones arithmeticæ* were published; the work was quickly recognized as one of the milestones in the history of the theory of numbers. From this point on his life was a series of most brilliant discoveries till his death at Göttingen, 1855.

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[1] Hänselmann, K. F. Gauss, Leipzig, 1878, p. 15.

[2] Hänselmann, K. F. Gauss, p. 25.

[3] His dissertation was entitled: *Demonstratio nova theorematis, omnem functionem algebraicum rationabilem integram unius variabilis in factores reales primi vel secundi gradus resolvi posse, Helmstedt, 1798.*

It is much to be regretted that no adequate life of Gauss has yet been written; nevertheless, the story of his discoveries is too well known[1] to need mention. We are here interested in his talent for calculation, for Gauss was not only a mathematical genius,—he was also an arithmetical prodigy, and that, too, at an age much earlier than any of the others.

An anecdote of his early life, told by himself, is as follows: His father was accustomed to pay his workmen at the end of the week, and to add on the pay for overtime, which was reckoned by the hour at a price in proportion to the daily wages. After the master had finished his calculations and was about to pay out the money, the boy, scarce three years old, who had followed unnoticed the acts of his father, raised himself and called out in his childish voice: "Father, the reckoning is wrong, it makes so much," naming a certain number. The calculation was repeated with great attention, and to the astonishment of all it was found to be exactly as the little fellow had said.[2]

At the age of nine Gauss entered the reckoning class of the town school. The teacher gave out an arithmetical series to be added. The words were scarcely spoken when Gauss threw his slate on the table, as was the custom, exclaiming, "There it lies!" The other scholars continue their figuring while the master throws a pitying look on the youngest of the scholars. At the end of the hour the slates were examined; Gauss's had only one number on it, the correct result alone.[3] At the age of ten he was ready to enter upon higher analysis. At fourteen he had become acquainted with the works of Euler and Lagrange, and had grasped the spirit and methods of Newton's Principia.

He was always distinguished for his power of reckoning, and was able to carry on difficult investigations and extensive numerical calculations with incredible ease. His unsurpassed memory for figures set those who met him in astonishment; if he could not answer a problem at once, he stored it up for

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[1] Except to Mr. Sully, who in an article "Genius and Precocity," in the *Nineteenth Century*, never even mentions Pascal, Ampère and Gauss.

[2] Waltershausen; Gauss zum Gedächtniss, Leipzig, 1856, p. 11.

[3] Hänselmann; Karl Friedrich Gauss, Zwölf Kapitel aus seinem Leben, Leipzig, 1878.

future solution. At once, or after a very short pause, he was able to give the properties of each of the first couple thousand numbers. In mental calculation he was unsurpassed. He had always in his mind the first decimals of all the logarithms, and used them for approximate estimates while calculating mentally. He would often pursue a calculation for days and weeks, and—what distinguishes him from all other calculators,—during such a calculation he continually invented new methods and new artifices.

Perhaps the best picture of his genius is given by Waltershausen: "Gauss showed a remarkable, perhaps unprecedented, combination of peculiar talents. To his eminent ability to work out in himself abstract investigations on all sides and from all standpoints, there were joined a marvellous power of numerical calculation, a peculiar sense for the quick apprehension of the most complicated relations of numbers, and an especial love for all exact observation of nature."[1]

From Gauss's opinion of Pfaff we get a hint of what he regarded as the essential of genius, "never to leave a matter till he had investigated wherever possible."

WHATELY.—Richard Whately, *1787, Archbishop of Dublin from 1831 to 1863, author of "Historic Doubts relative to Napoleon Bonaparte," "Elements of Logic," "Elements of Rhetoric," and numerous other works, mostly religious, displayed a singular precocity in regard to calculation. At six years old he astonished his family by telling Parkhurst, a man of past sixty, how many minutes he was old.

"There certainly was something peculiar in my calculating faculty," wrote Whately in his Commonplace Book. "It began to show itself between five and six, and lasted about three years. One of the earliest things I can remember is the discovery of the difference between even and odd numbers; . . . I soon got to do the most difficult sums, always in my head, for I knew nothing of figures beyond numeration, nor had I any names for the different processes I employed. But I believe my sums were chiefly in multiplication, division and the rule of three. In this last point I believe I surpassed the famous American boy, though I did

[1] Waltershausen, Gauss, p. 83.

not, like him, understand the extraction of roots. I did these sums much quicker than any one could upon paper, and I never remember committing the smallest error." . . .

"When I went to school, at which time the passion was worn off, I was a perfect dunce at cyphering, and so have continued ever since."[1]

ZERAH COLBURN.[2]—Autobiographies do not always furnish the most trustworthy evidence in regard to the man himself; when, moreover, the author is convinced that he is nothing less than a modern miracle; and, finally, when having had no scientific and little literary education, he at a later date writes the memoirs of his youth, we are obliged to supply the lacking critical treatment of the narrative. The main source of information in regard to Colburn's youthful powers consists of his memoirs published by him in 1833.[3] Only one contemporary account of his earliest exhibitions in America isto be found, we must rely mostly on his own statements, probably derived from recollections of his friends, and on a "Prospectus," a sort of advertisement, published in London in 1813.

Zerah Colburn,* 1804,† 1840, of Cabot, Vt., was considered a very backward child. In the year 1810, a short time after

[1] Jane Whately, Life and Correspondence of Richard Whately, D. D., London, 1866, I, 4.

[2] Bibliography; A Memoir of Zerah Colburn, written by himself, Springfield, Mass.,1 833; Medical and Philosophical Journal and Review, New York, 1811, Vol. III, p. 19; Philosophical Magazine, XL., London, 1812, p. 119; Philosophical Magazine, XLII, London, 1813. p. 481 [report of the proceedings of the Royal Society, in which a letter concerning the extra digits on members of the Colburn family was read]; Analectic Magazine, Vol. I, year 1813, p. 124 ff. [contains a reprint of an article by the calculator, Mr. Francis Baily, in the Literary Panorama, Oct. 1812, which article is almost identical with the one in the Philosophical Magazine, 1812, p. 119.] Graves; Life of Sir William R. Hamilton, I, 78 ff.; American Almanac, 1840, p. 307. In Faraday's commonplace book there is an unpublished account of Zerah Colburn, who visited Faraday in 1816 and explained to him his method of calculation; Jones, Life of Faraday, London, 1870, I, 221. Gall, Functions of the Brain, Organology, XVIII; R. A. Proctor, On Some Strange Mental Feats, Cornhill Mag., Aug. 1875, Vol. XXXII, p. 157, reprinted in Science Byways, London, 1875, p. 337. [This is an attempt at explaining Colburn's powers; the objections to it will be found below.] R. A. Proctor, Calculating Boys, in Belgravia, Vol. XXXVIII, p. 450 [contains a further explanation.] Carpenter, Mental Physiology, Chap. VI, § 205 [quoting from Baily's account.]

[3] There is no statement regarding the time at which they were written, or even a date to the preface; the last year mentioned in the book is 1827.

a six weeks attendance at the district school, in which he had learned no arithmetic [unless from the recitations of other boys in the class-room], his father heard him saying "5 times 7 are 35," "6 times 8 are 48," etc., and upon examining him and finding him perfect in the multiplication table, he asked the product of 13×97, to which 1261 was instantly given in answer. The account given by Zerah himself, when stated in plain terms, amounts to this; nevertheless, one is tempted to ask for the authority on which the statements were made. If Zerah remembered the exact figures himself till the time of writing his memoirs, then his power of memory for long periods must have been extraordinary, yet he never mentions such powers. On the other hand, if these statements are made from the stories current about him, the general untrustworthiness of such evidence does not allow us to put too much faith in the figures.

Before long Zerah's father took him to Montpelier, Vt., where he was exhibited. Of his performances here Colburn gives only three specimens. "Which is the most, twice twenty-five, or twice five and twenty (2×25 or $2 \times 5 + 20$?) Ans.—Twice twenty five. Which is the most, six dozen dozen, or half a dozen dozen ($6 \times 12 \times 12$ or 6×12?) Ans.—6 dozen dozen. It is a fact, too, that somebody asked how many black beans would make five white ones? Ans.—5, if you skin them."[1] It is at once apparent that these questions do not demand any extraordinary calculating powers, but on the other hand, a sharpness of wit and an analytical quickness of comprehending puzzles that would be phenomenal in a joker and riddle-maker of ripe years. If it is really true that the child answered the last of these questions, then the real miracle is that he should on not a single other occasion of his life have shown a sign of the Yankee quickness and shrewdness here implied.

On the journey to Boston, Zerah's wonderful gifts convinced A. B., Esq., that "something had happened contrary to the course of nature and far above it;" he was compelled by this "to renounce his Infidel foundation, and ever since has been established in the doctrines of Christianity." At

[1] Memoirs, p. 12.

Boston he gave public exhibitions. "Questions in multiplication of two or three places of figures, were answered with much greater rapidity than they could be solved on paper. Questions involving an application of this rule, as in Reduction, Rule of Three, and Practice, seemed to be perfectly adapted to his mind." The extraction of the roots of exact squares and cubes was done with very little effort; and what has been considered by the Mathematicians of Europe an operation for which no rule existed, viz., finding the factors of numbers, was performed by him, and in course of time he was able to point out his method of obtaining them. "Questions in Addition, Subtraction and Division were done with less facility, on account of the more complicated and continued effort of the memory [sic.] In regard to the higher branches of Arithmetic, he would observe that he had no rules peculiar to himself; but if the common process was pointed out as laid down in the books, he would carry on the process very readily in his head."[1]

Among the questions answered at Boston were the following:[2] "The number of seconds in 2000 years was required?"

$$\left.\begin{array}{r}\text{730,000 days,}\\ \text{17,520,000 hours,}\\ \text{1,051,200,000 minutes,}\\ \text{63,072,000,000 seconds,}\end{array}\right\}\text{ Answer.}$$

"Supposing I have a corn-field, in which are 7 acres, having 17 rows to each acre; 64 hills to each row; 8 ears on a hill, and 150 kernels on an ear; how many kernels on the corn-field? Answer, 9,139,200."

At this time he was a child only six years old, unable to read and ignorant of the name or properties of one figure traced on paper. The exercise of his faculty under such circumstances causes him later to exclaim: "for it ever has been, and still is, as much a matter of astonishment to him as it can be to any other one; God was its author, its object and aim are perhaps still unknown."[3]

Shortly afterward, on a steamboat journey up to Albany, a gentleman taught Zerah the names and the powers of the

[1] Memoirs, p. 15.
[2] P. 171, of the Memoirs, perhaps on the authority of the London Prospectus mentioned above, although Colburn does not say so.
[3] Memoirs, p. 15.

nine units, of which he had been previously ignorant. In June, 1811, he visited Portsmouth and answered the following: "Admitting the distance between Concord and Boston to be 65 miles, how many steps must I take in going this distance, allowing that I go three feet at a step? The answer, 114,400, was given in ten seconds. "How many seconds in eleven years? Answer, in four seconds, 346,896,000. What sum multiplied by itself will produce 998,001? In less than four seconds, 999."[1]

Next summer Zerah's father took him to England and made efforts to secure the patronage of the nobility. At a meeting of his friends "he undertook and succeeded in raising the number 8 to the sixteenth power, 281,474,976,710,656. He was then tried as to other numbers, consisting of one figure, all of which he raised as high as the tenth power, with so much facility that the person appointed to take down the results was obliged to enjoin him not to be too rapid. With respect to numbers of two figures, he would raise some of them to the sixth, seventh and eighth power, but not always with equal facility; for the larger the products became the more difficult he found it to proceed. He was asked the square root of 106,929, and before the number could be written down he immediately answered 327. He was then requested to name the cube root of 268,336,125, and with equal facility and promptness he replied 645 [Extracted from a Prospectus printed in London, 1813]."[2]

"It had been asserted . . . . that 4,294,967,297 ($=2^{32}+1$) was a prime number. . . . Euler detected the error by discovering that it was equal to 641 $\times$ 6,700,417. The same number was proposed to this child, who found out the factors by the mere operation of his mind. [Ibid.]"[3]

Colburn is undoubtedly the one referred to as the Russian boy in the Gentleman's Magazine of 1812. He showed him-

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[1] Memoirs, p. 171.
[2] Memoirs, p. 37.
[3] Memoirs, p. 38. It requires considerable faith to accept this statement, although S. B. Morse met him in London, and a friend of Morse writes that "There was some great arithmetical question, I do not exactly know what, which he solved almost as soon as it was put to him, though it for several years baffled the skill of some of the first professors." Prime, Life of S. B. Morse, New York, 1875, p. 68.

self to the merchants of the London Stock Exchange; one of them gave the boy a guinea of William III, and demanded to know how many years, months and days had elapsed since its coinage; all of which he answered promptly.[1] This is confirmed by a passage in a letter from a friend of S. B. Morse: "Zerah Colburn . . . has called on us. . . . He has excited much astonishment here, and, as they are very unwilling just at this time to allow any cleverness to the Americans, it was said in some of the papers that he was a Russian."[2]

The father and son, after a visit to Ireland and Scotland, returned to London. In 1814 they proceeded to Paris, where the people manifested very little interest in his calculations. This neglect he can only explain by a national defect of character or a crushing historical event. "Whether it were principally owing to the native frivolity and lightness of the French people, or to the painful effect produced by the defeat of their armies and the restoration of the exiled Louis XVIII, cannot be correctly stated; probably it was owing to the former, etc."[3]

He was introduced to and examined by the members of the French "Institute," among whom was La Place. "Three months had now elapsed that he had not been exhibited, but had given his attention to study; even in this short space it was observable that he had lost in the quickness of his computations."[4] Before long his calculating power left him entirely.

By the exertions of Washington Irving, at that time in Paris, the boy obtained admission to the Lyceum Napoleon (or Royal College of Henri IV.) Zerah gives an interesting account of this institution, which was under strict military discipline, and also of Westminster School, in which he was placed on his return to England.

Being in financial straits the father suggests the stage, and so Zerah makes an unsuccessful attempt at acting. There-

[1] Gentleman's Magazine, 1812, Vol. LXXXII, Pt. II, p. 584.
[2] Prime, Life of S. B. Morse, New York, 1875, p. 68.
[3] Memoirs, p. 74.
[4] Memoirs, p. 76.

after, in 1821, he starts a private school, which was given up after somewhat more than a year. After his return to America he joined the Congregational church, but soon went over to the Methodists and began to hold religious meetings. He was ordained deacon, and labored thenceforth as an itinerant preacher, till, in 1835, he was appointed "Professor of the Latin, Greek, French and Spanish Languages, and English Classical Literature in the seminary styled the Norwich University."[1] Here he died at the age of 35, leaving a wife and three children.

It is to be remarked that Colburn's calculating powers, such as they were, seemed to have absorbed all his mental energy; he was unable to learn much of anything, and incapable of the exercise of even ordinary intelligence or of any practical application. The only quality for which he was especially distinguished was self-appreciation. He speaks, for example, of Bidder as "the person who approached the nearest to an equality with himself in mental arithmetic."[2] Again, "he thinks it no vanity to consider himself first in the list in the order of time, and probably first in the extent of intellectual power."[3]

Colburn possessed bodily as well as mental peculiarities. His father and great-grandmother had a supernumerary digit on each hand and each foot; Zerah and three (or two?[4]) brothers possessed these extra members, while they were wanting in two brothers and two sisters. These digits are attached to the little fingers and little toes of the hands and feet, each having complete metacarpal and metatarsal bones.[5] Zerah leaves it a matter of doubt "whether this be a proof of direct lineal descent from Philistine blood or not (see 1 Chronicles xx. 6)."[6] A portrait of Colburn was made in Philadelphia in 1810, and placed in the museum,[7] and another

[1] American Almanac, 1840, p. 307, where he is spoken of as Rev. Zerah Colburn. The University of Norwich (Vt.), after a fire in 1866, was removed to Northfield, Vt.
[2] Memoirs, p. 175.
[3] Memoirs, p. 176.
[4] Memoirs, p. 72.
[5] Philos. Mag. XLII, 481.
[6] Memoirs, p. 72.
[7] Memoirs, p. 20.

was engraved in London in 1812. The origin of the portrait prefixed to his memoirs is not given; it shows a large head, with unusual development of the upper parts; the forehead is rather small and angular, the occiput is small;[1] the eyes are quite large with projecting orbital arch. Gall, who examined the boy without any previous intimation of his character, "readily discovered on the sides of the eyebrows certain protuberances and peculiarities which indicated the presence of a faculty for computation."[2]

MANGIAMELE.—In the year 1837 Vito Mangiamele, who gave his age as 10 years and 4 mos., presented himself before Arago in Paris. He was the son of a shepherd of Sicily, who was not able to give his son any instruction. By chance it was discovered that by methods peculiar to himself, he resolved problems that seemed at the first view to require extended mathematical knowledge. In the presence of the Academy Arago proposed the following questions: "What is the cubic root of 3,796,416? In the space of about half a minute the child responded 156, which is correct. What satisfies the condition that its cube plus five times its square is equal to 42 times itself increased by 40? Everybody understands that this is a demand for the root of the equation: $x^3 + 5x^2 - 42x - 40 = 0$. In less than a minute Vito responded that 5 satisfied the condition; which is correct. The third question related to the solution of the equation: $x^5 - 4x - 16779 = 0$. This time the child remained four to five minutes without answering; finally he demanded with some hesitation if 3 would not be the solution desired. The secretary having informed him that he was wrong, Vito, a few moments afterwards, gave the number 7 as the true solution. Having finally been requested to extract the 10th root of 282,475,249, Vito found in a short time that the root is 7."[3] At a later date a committee, composed of Arago, Cauchy and others, complains that "the masters of Mangiamele have always kept secret the methods of calculation which he made use of."[4]

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[1] Medical and Philos. Journal and Rev., N. Y., 1811, Vol. III, p. 21

[2] Memoirs, p. 77.

[3] Comptes rendus des séances de l'Academie des Sciences, 1837, IV, 978.

[4] Comptes rendus, etc., 1840, XI, 952; reprinted in Oeuvres complétes de A. Cauchy, Paris, 1885, Ire serie, tome V, p. 493.

ZACHARIAS DASE.[1]—Zacharias Dase (also, Dahse) *1824, †1861, was born with a natural talent for reckoning; in his own opinion his early instruction had very little influence on him; but his powers were later developed by practice and industry.[2] He spent most of his life in Hamburg, but made many journeys through Germany, Denmark and England, giving exhibitions in ready reckoning in the most important towns. He became acquainted with many learned men, among whom were Gauss, Schumacher, Petersen, Encke, et al. On one occasion Petersen tried in vain for six weeks to get the first elements of mathematics into his head. Schumacher credits him with extreme stupidity.

In 1840, Dase exhibited in Vienna. He attended the lectures of Prof. Strasznicky on the elements of mathematics, who seems to have brought him to such a point that under the guidance of a good mathematician he could do scientific work. He was induced to reckon out the value of π, which he did in two months with the formula $\frac{1}{4}\pi = \text{arc tang } \frac{1}{2} + \text{arc tang } \frac{1}{8} + \text{arc tang } \frac{1}{5}$. The result, which is published in Crelle's Journal (loc. cit.), agreed with that of Thibaut. In 1844, he had a position in the Railroad Department at Vienna; in 1845 he appears in Mannheim; in 1846 he seems to have had a position in Berlin.

Dase was ambitious to make some use of his powers in the service of science. In 1847 he had reckoned out the natural logarithms (7 places) of the numbers from 1 to 1,005,000, and was seeking a publisher.[3] In reckoning on paper he possessed all the accuracy of mental calculation, and added to

[1] Bibliography: Briefwechsel zwischen Gauss und Schumacher, herausg. von Peters, Altona, 1861. III, 382; V, 30, 32, 277, 278, 295, 296, 297, 298, 300, 301, 302, 303, 304; VI, 27, 28, 78, 112. Crelle; Journal für Mathematik, 1844, vol. XXVII, p. 198. Dase; Factoren-Tafel für alle Zahlen der siebenten Million, Hamburg, 1862; (the introduction contains remarks on Dase and a letter from Gauss.) Schröder's Lexikon der hamburgischen Schriftsteller, Hamburg, 1851, v. Dase. Auszug aus dem Album des Zacharias Dase, Wien, 1850. Anhang dazu, Hamburg, 1850. Preyer, Counting Unconsciously, in Pop. Science Monthly, XXIX, p. 221; reprinted from Die Gartenlaube, 1886, Bd. I. Littell's Living Age, 1857, LIV, p. 61, "On Mental Calculation"; reprinted from the Atheneum. Accounts of Dase are given in two periodicals at present not accessible to me: Allgemeine Literatur-Zeitung, 1861; Zeitschrift für österreichische Gymnasien, vol. XII.

[2] Schröder's Lexikon gives the account of Dase "nach Selbstbericht."

[3] Briefwechsel zw. Gauss und Schumacher, V, 277.

this an incredible rapidity in doing long problems. In the same year he had completed the calculations for the compensation of the Prussian triangulations. In 1850 the largest hyperbolic table, as regards range, was published by him at Vienna, under the title, "*Tafel der natürlichen Logarithmen der Zahlen*"; the same was reprinted in the annals of the Vienna observatory.[1]

In 1850 Dase went to England to earn money by exhibitions of his talents. Much the same is related of his great powers as in Germany; his general obtuseness also occasioned remark. He could not be made to have the least idea of a proposition in Euclid. Of any language but his own he could never master a word.

In 1849 Dase had wished to make tables of factors and prime numbers from the 7th to the 10th million. The Academy of Sciences at Hamburg was ready to grant him support, provided Gauss considered the work useful. Gauss writes him: "With small numbers, everybody that possesses any readiness in reckoning, sees the answer to such a question [the divisibility of a number] at once directly, for greater numbers with more or less trouble; this trouble grows in an increasing relation as the numbers grow, till even a practiced reckoner requires hours, yes days, for a single number; for still greater numbers, the solution by special calculation is entirely impracticable. . . . You possess many of the requisite qualities [for establishing tables of factors] in a special degree, a remarkable agility and quickness in handling arithmetical operations, . . . and an invulnerable persistence and perseverance."[2] The assistance was granted and Dase gave himself up to the execution of the task. Up to his death, in 1861, he had completed the 7th million and also the 8th, with the exception of a small portion. Thus he was able to turn his only mental ability to the service of science, forming a contrast to Colburn and Mondeux, who enjoyed even greater advantages yet failed to yield any results.

[1] Tafel der natürlichen Logarithmen, in Annalen der K. K. Sternwarte in Wien, Theil 34, neuer Folge Bd. XIV, Wien, 1851.

[2] Gauss's letter is given in the preface to Dase's Factoren-Tafeln, 7te Million, Hamburg, 1862.

He multiplied and divided large numbers in his head, but when the numbers were very large he required considerable time. Schumacher once gave him the numbers 79,532,853 and 93,758,479 to be multiplied. From the moment in which they were given to the moment when he had written down the answer, which he had reckoned out in his head, there elapsed 54 seconds.[1] He multiplied mentally two numbers each of 20 figures in 6 minutes; 40 figures in 40 minutes; and 100 figures in 8¾ hours, which last calculation must have made his exhibitions somewhat tiresome to the onlookers. He extracted mentally the square root of a number of 100 figures in 52 minutes.

It is curious that although Dase generally reckoned with astonishing accuracy, yet on at least two occasions his powers failed him. While he was in Hamburg, in 1840, he gave striking proofs of his talents, but at times made great mistakes, which luckily for him happened seldomer than his correct answers.[2] In 1845, Schumacher writes, "at a test which he was to undergo before me, he reckoned wrongly every time." This was explained as coming from a headache.

He had one ability not present to such a great degree in the other ready reckoners. He could distinguish some thirty objects of a similar nature in a single moment as easily as other people can recognize three or four. The rapidity with which he would name the number of sheep in a herd, of books in a book-case, of window-panes in a large house, was even more remarkable than the accuracy of his mental calculations.

Prolongeau.—A committee of the Academy of Sciences of Paris, including Arago and Cauchy, undertook in 1845 to investigate the powers of a child of 6½ years, who possessed an extraordinary aptitude for calculation. "He solves mentally with great facility problems relating to the ordinary operations of arithmetic and to the solution of equations of the first degree."[3]

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[1] Briefwechsel zw. Gauss und Schumacher, V, 302.
[2] Briefwechsel zw. Gauss und Schumacher, III, 382.
[3] Comptes rendus des séances de l'Academie des Sciences, 1845, t. XX, p. 1629.

GRANDMANGE.—In 1852 the attention of the Academy of Sciences of Paris was called to a young man of 16 years, C. Grandmange, born without legs or arms, who performed mentally very complicated calculations and solved difficult problems.[1] The committee appointed to investigate the case seems never to have reported.

MONDEUX.—Henri Mondeux,[2] *1826, †1862, was the son of a poor wood-cutter in the neighborhood of Tours. Sent at the age of seven to keep sheep, and deprived of all instruction, he amused himself in counting and arranging pebbles. At this period of life pebbles seem to have been his signs for numbers, for he was ignorant of figures. He learned to execute arithmetical operations mentally and to create for his own use ingenious methods of simplification. After long exercise at this calculation, he used to offer to persons he met to solve certain problems such as to tell how many hours or minutes were contained in the number of years which expressed their ages. This awakened the interest of M. Jacoby, a schoolmaster at Tours, who sought him out. Jacoby proposed several problems and received immediate answers and, finding that the boy could neither read, write nor cipher, and that he had no acquaintance with fractions or any of the ordinary rules of arithmetic, he offered to instruct him. Unfortunately the mind that could carry so many figures could not remember a name or an address, so the boy spent a month searching the city before he found his benefactor. He received instruction in calculation and was often shown in neighboring colleges and schools.

Although in other matters he showed only mediocre intelligence, yet he was something more than a mere calculating machine, as is shown for example in his way of solving the

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[1] Comptes rendus, etc., 1852, t. XXXIV, p. 371.

[2] Bibliography: Comptes rendus des séances de l'Academie des Sciences, XI, p. 820, 952. Oeuvres complètes de Cauchy, Ire Série, Paris, 1885, t. V, p. 493, (this is a reprint of Cauchy's report in the Comptes rendus.) Barbier, Vie d'Henri Mondeux, 1841; Jacoby, Biographie d'Henri Mondeux, 1846; Jacoby, la Clef de l'Arithmetique, 1860; Larousse, Dictionaire universelle, v. Mondeux; Didot's Nouvelle biographie générale, v. Mondeux; The Story of a Wonderful Boy Mathematician, in Every Saturday p. 118, vol. XI, June to Dec. 1871; Biographie universelle, Michaud, v. Mondeux.

following problem: "In a public square there is a fountain containing an unknown quantity of water; around it stands a group of people with vessels capable of containing a certain unknown quantity. They draw at the following rate: The first takes 100 quarts and $\frac{1}{13}$ of the remainder; the second 200 quarts and $\frac{1}{13}$ of the remainder; the third 300 quarts and $\frac{1}{13}$, and so on until the fountain was emptied. How many quarts were there? In a few seconds he gave the answer, and this is the simple process by which he obtained it: Take the denominator of the fraction, substract one; that gives the number of persons. Multiply that by the number of quarts taken by the first person—that is, by 100—and you get the equal quantities taken by each; square this number and multiply by the number of quarts, and you get the quantity in the fountain."[1]

In 1840, M. Jacoby presented the boy to the Academy of Sciences of Paris. Jacoby had taken note of the processes employed, and the boy was willing to unfold them himself before a commission. On this occasion two questions were given him, one of which was this: "How many minutes in 52 years? The child, who found the problem very simple, responded in a few moments: 52 years of 365 days each, are composed of 27,331,200 minutes, and of 1,639,872,000 seconds."[2] A committee, including Arago and Cauchy, made an exhaustive examination of his powers and reported on the processes used by him. "At present he easily executes in his head not only diverse operations of arithmetic, but also in many cases the numerical resolution of equations: he invents processes, sometimes remarkable, to solve various questions which are ordinarily treated with the aid of algebra."[3] In spite, however, of his marvellous power of inventing and applying arithmetical methods, he did not answer the expectations of his friends, but sank into obscurity and died almost unknown.

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[1] Every Saturday, XI, 118.
[2] Comptes rendus, etc., 1840, t. XI, p. 820.
[3] Comptes rendus, etc., 1840, XI, 953.

GEORGE BIDDER.—Geo. Bidder,[1] *1806, †1878, was the son of an English stonemason. His first and only instruction in numbers was received at about 6 years of age, from his elder brother, from whom he learned to count up to 10 and then to 100.

"I amused myself," he says, "by repeating the process [of counting up to 100], and found that by stopping at 10, and repeating that every time, I counted up to 100 much quicker than by going straight through the series. I counted up to 10, then to 10 again=20, 3 times 10=30, 4 times 10=40, and so on. This may appear to you a simple process, but I attach the utmost importance to it, because it made me perfectly familiar with numbers up to 100; . . . at this time I did not know one written or printed figure from another, and my knowledge of language was so restricted, that I did not know there was such a word as 'multiply'; but having acquired the power of counting up to 100 by 10 and by 5, I set about, in my own way, to acquire the multiplication table. This I arrived at by getting peas, or marbles, and at last I obtained a treasure in a small bag of shot: I used to arrange them in squares, of 8 on each side, and then on counting them throughout I found that the whole number amounted to 64: by that process I satisfied my mind, not only as a matter of memory, but as a matter of conviction, that 8 times 8 were 64; and that fact once established has remained there undisturbed until this day, . . . in this way I acquired the whole multiplication table up to 10 times 10; beyond which I never went; it was all that I required."[2]

Most of the child's time was spent with an old blacksmith.

[1] Bibliography: Proceedings of the Institution of Civil Engineers, vol. XV., session 1855-56, London, 1856, p. 251 ff., "On Mental Calculation"; Vol. LVII, session 1878-79, part III, London, 1879, p. 294, "Memoirs of Deceased Members." A Memoir of Zerah Colburn, written by himself, Springfield, 1833, p. 175. Philosophical Magazine, XLVII, London, 1816, p. 314. Reviews of Bidder's speech are given in Littell's Living Age, 1856, XLIX, p. 254; 1857, LIV, p. 61. A correspondent in the Spectator, 1879, LII, p. 111, quotes from a pamphlet in his possession, the title-page of which is missing. The printers of this pamphlet were M. Bryan & Co., Bristol, and the date is estimated to be 1820. It contains a large number of questions proposed to Bidder at various places in the years 1816-19; the answers given are appended, often with the time it took him to perform the operation.

[2] Proceedings Civ. Eng., XV, p. 257.

On one occasion somebody by chance mentioned a sum and the boy astonished the bystanders by giving the answer correctly. "They went on to ask me up to two places of figures, 13 times 17 for instance; that was rather beyond me at the time, but I had been accustomed to reason on figures, and I said 13 times 17 means 10 times 10 plus 10 times 7, plus 10 times 3 and 3 times 7. . . . . ."[1]

While remaining at the forge he received no instruction in arithmetic beyond desultory scraps of information derived from persons who came to test his powers, and who often in doing so gave him new ideas and encouraged the further development of his peculiar faculty, until he obtained a mastery of figures that appeared almost incredible. "By degrees I got on until the multiple arrived at thousands. Then . . . . it was explained to me that 10 hundreds meant 1000. Numeration beyond that point is very simple in its features; 1000 rapidly gets up to 10,000 and 20,000, as it is simply 10 or 20 repeated over again, with thousands at the end, instead of nothing. So by degrees I became familiar with the numeration table, up to a million. From two places of figures I got to three places; then to four places of figures, which took me up of course to tens of millions; then I ventured to five and six places of figures, which I could eventually treat with great facility, and as already mentioned, on one occasion I went through the task of multiplying 12 places of figures by 12 figures, but it was a great and distressing effort."[2]

Before long he was taken about the country by his father for the purpose of exhibition. This was so profitable for the father that the boy's education was entirely neglected. Even at the age of ten he was just learning to write; figures he could not make. Some of the questions he had answered were the following: "Suppose a cistern capable of containing 170 gallons, to receive from one cock 54 gallons, and at the same time to lose by leakage 30 gallons in one minute; in what time will the said cistern be full?" "How many drops are there in a pipe of wine, supposing each cubic inch to contain 4685 drops, each gallon 231 inches and 126 gallons

[1] Proceedings Civ. Eng., XV, p. 258.
[2] Proceedings Civ. Eng., XV, p. 259.

in a pipe?" "In the cube of 36, how many times 15228?"[1] Among others the famous Herschel came in 1817 to see the "Calculating Boy."

Shortly afterward he was sent to school for a while. Later he was privately instructed, and then attended the University of Edinburgh, obtaining the mathematical prize in 1822. Later he entered the Ordnance Survey, and then was employed by the Institution of Civil Engineers. He was engaged in several engineering works of importance; he is also to be regarded as the founder of the London telegraphic system. His greatest work was the construction of the Victoria (London) Docks. Bidder was engaged in most of the great railway contests in Parliament, and was accounted "the best witness that ever entered a committee room." He was a prominent member, Vice President, then President of the Institution of Civil Engineers. In his later years there was no appreciable diminution in Bidder's powers of retaining statistics in his memory and of rapidly dealing with figures. Two days before his death the query was suggested that taking the velocity of light at 190,000 miles per second, and the wave length of the red rays at 36,918 to an inch, how many of its waves must strike the eye in one second. His friend, producing a pencil, was about to calculate the result, when Mr. Bidder said, "You need not work it; the number of vibrations will be 444,433,651,200,000."[2]

The fact that Bidder became a highly educated man, and one of the leading engineers of his time; that his powers increased rather than diminished with age; and above all, that he has given a clear and trustworthy account of how he obtained and exercised his talent, renders his testimony of the highest worth, and provides the solution of many of the dark problems met with in the cases of Dase, Colburn, and others. Indeed, he seems to fill out just what is lacking in each case; Dase never gave a good account of the way in which he worked; Colburn could not till later explain his methods, and then only in the clumsy way to be expected from a young man of little education; finally, just the part we cannot understand in Buxton is here explained in full.

[1] Philos. Mag. XLVII, p. 315.
[2] Proceedings Civ. Eng., LVII, 309.

In 1814 a witness to his powers states that he displayed great facility in the mental handling of numbers, multiplying readily and correctly two figures by two, but failing in attempting numbers of three figures. This same witness was present at an examination of the boy in 1816 by several Cambridge men. The first question was a sum in simple addition, two rows with twelve figures in each row; the boy gave the correct answer immediately. After more than an hour the question was asked, "Do you remember the sum in addition I gave you?" He repeated the twenty-four figures with only one or two mistakes. At this time he could not explain the processes by which he worked out long and intricate sums. "It is evident that in the course of two years his powers of memory and calculation must have been gradually developed."[1]

This development seems to have been steady. The following series shows the increasing rapidity with which the answers came:

1816 (10 years of age). What is the interest of £4,444 for 4,444 days, at $4\frac{1}{2}\%$ per annum? Ans. in 2 min., £2,434, 16*s.* $5\frac{1}{4}$*d.*

1817 (10 years of age). How long would a cistern 1 mile cube be filling, if receiving from a river 120 gallons per minute without intermission? Ans. in 2 minutes—years 14,300, days 285, hours 12, minutes 46.

1818 (11 years of age). Divide 468,592,413,563 by 9,076. Ans. within 1 min., 51,629,838.

1818 (12 years of age). If the pendulum of a clock vibrates the distance of $9\frac{3}{4}$ inches in a second of time, how many inches will it vibrate in 7 years, 14 days, 2 hours, 1 minute, 56 seconds, each year being 365 days, 5 hours, 48 minutes, 55 seconds? Ans. in less than a minute, 2,165,625,744$\frac{3}{4}$ inches.

1819 (13 years of age). To find a number whose cube less 19 multiplied by its cube shall be equal to the cube of 6. Ans. instantly, 3.[2]

Sir Wm. Herschel put the following question to the boy:

[1] Spectator, 1879, LII, p. 47.
[2] Spectator, 1879, LII, p. 111.

Light travels from the sun to the earth in 8 minutes, and the sun being 98,000,000 miles off, if light would take 6 years and 4 months traveling at the same rate from the nearest fixed star, how far is that star from the earth, reckoning 365 days and 6 hours to each year, and 28 days to each month? Ans., 40,633,740,000,000 miles.[1]

Curious enough is the fact that Bidder and Colburn met in Derbyshire, and underwent a comparative examination, the result of which is said to have been to the total defeat of Colburn.[2]

Prof. Elliot, of Liverpool, who knew Bidder from the time they were fellow-students in Edinburgh, says he was a man of first-rate business ability and of rapid and clear insight into what would pay, especially in railway matters. As a proof of this statement we can accept the fact that Bidder became a wealthy man.

The Bidder family seem to have been distinguished for mental traits resembling George Bidder's in some part or another. Bidder was noted for his great mathematical ability and his great memory. One of his brothers was an excellent mathematician and an actuary of the Royal Exchange Life Assurance Office.[3] Rev. Thomas Threlkeld, an elder brother, was a Unitarian minister. He was not remarkable as an arithmetician, but he possessed the Bidder memory and showed the Bidder inclination for figures, but lacked the power of rapid calculation. He could quote almost any text in the Bible, and give chapter and verse.[4] He had long collected all the dates he could, not only of historical persons, but of everybody; to know when a person was born or married was a source of gratification to him.[5]

One of George Bidder's nephews at an early age possessed remarkable mechanical ingenuity.

Most interesting of all is the partial transmission of his peculiar faculties to his son, George Bidder, Q. C., and through him to two grandchildren. The second son was a

[1] Spectator, 1879, LII, p. 112.
[2] Spectator, 1879, LII, p. 112.
[3] Spectator, 1878, LI, p. 1635.
[4] Spectator, 1878, LI, p. 1635.
[5] Brierly's Journal, Jan. 25, 1879, quoted in the Spectator, 1879, LII p. 143.

first-class man in classics at Oxford, and Fellow of his college. The elder Bidder, however, possessed the peculiar faculties of the family is such proportions that he far exceeded the others in calculating powers.

GEORGE BIDDER, Q. C.—Bidder's calculating faculty was transmitted to his eldest son. It has caused some confusion that he bore the same name as his father. Some writers have lately referred to the father as G. P. Bidder, but since he was always known as Geo. Bidder, the only way out of the difficulty is to distinguish the son by adding his title.

George Bidder, Q. C., distinguished himself at Cambridge in mathematics, being seventh wrangler of his year. He is now a thriving barrister and Queen's Counsel.

He possesses a remarkable visual memory. He always sees mental pictures of figures and geometrical diagrams. "If I perform a sum mentally it always proceeds in a visible form in my mind; indeed, I can conceive no other way possible of doing mental arithmetic."[1]

He considers the special aids to mental calculation to be a powerful memory of a peculiar cast, in which figures seem to stereotype themselves without an effort, and an almost inconceivable rapidity of operation. The former he possessed in a high degree; the latter was no doubt congenital, but was developed by incessant practice and by the confidence thereby acquired.

Bidder says: "I myself can perform pretty extensive arithmetical operations mentally, but I cannot pretend to approach even distantly to the rapidity and accuracy with which my father worked. I have occasionally multiplied 15 figures by 15 [figures] in my head, but it takes me a long time and I am liable to occasional errors." Just before writing this he tried the following to see if he could still do it:

378,201,969,513,825
199,631,057,265,413

"I got in my head the answer 75,576,299,427,512,145,197,597,834,725, in which I think you will find four figures out of the 29 are wrong."[2]

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[1] Spectator, 1878, LI, p. 1634.
[2] Spectator, 1878, LI, p. 1635.

We have no account from George Bidder, Q. C., to show whether he performs the operations rapidly or not.

The daughters of Mr. Bidder, Q. C., show more than the average but not extraordinary powers of doing mental arithmetic. To test their calculating powers Prof. Elliot in 1877 asked them, "At what point in the scale do Fahrenheit's thermometer and the centigrade show the same number at the same temperature?" The nature of the two scales had to be explained, but after that they were left to their own resources. The next morning one of the younger ones (about ten years old) said it was at 40 degrees below zero. This is the correct answer; she had worked it out in bed.[1]

Another granddaughter shows great visual memory. On one occasion she remarked, "When I hear anything remarkable read or said to me, I think I see it in print."

SAFFORD.[2]—Truman Henry Safford was born at Royalton, Vt.,[3] in 1836. Even in his earlier years his parents had amused themselves with his power of calculating. When six years of age he told his mother that if he knew how many rods it was round his father's large meadow he could tell the measure in barley-corns; on hearing that it was 1040 rods, he gave, after a few minutes the answer, 617,760, which he had reckoned out in his head. Before his eighth year he had gone to the extent of Colburn's powers. His abilities were won by means of study, and it was observed that he improved rapidly by practice and lost by neglect.

In 1845, Dr. Dewey wrote of him, "He is a regular reasoner on correct and established principles, taking the easiest and most direct course. As he had Hutton's Mathematics, and wanted some logarithms, his father told me he computed the logarithms from 1 to 60 by the formula given

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[1] Spectator, 1878, LI, p. 1634.

[2] Bibliography: Appleton's Cyclopedia of American Biography, v. Safford; Chamber's Edinburgh Journal, July-Dec. 1847, Vol. VIII, p. 265, article "Truman Henry Safford," (this is founded on an article in the "Christian Alliance and Family Visitor," of Boston); Littell's Living Age, XVI, p. 82, has copied the article from Chamber's Journal; Leisure Hour, I, p. 540, contains an abstract of the same article.

[3] It is a curious fact that Safford was born within 40 miles of Colburn's birthplace, and 15 of Norwich.

by Hutton, which were afterwards found to be the same in a table of logarithms for the same number of decimals."[1]

In his return from a little tour, in which he had been introduced to various scientific men, he set about constructing an almanac which was put to press when the author was just 9½ years old. In the following year he calculated four different almanac calendars. While getting up the Cincinnati one he originated a new rule for getting moon-risings and settings, accompanied by a table which saves full one-fourth of the work in casting moon-risings. This rule and the manuscript almanacs are preserved in the Harvard library, as are also his new rules for calculating eclipses. At ten years of age he was carefully examined by Rev. H. W. Adams, with questions prepared beforehand. Adams says: "I had only to read the sum to him once. . . . Let this fact be remembered in connection with some of the long and blind sums I shall hereafter name, and see if it does not show his amazing power of conception and comprehension."[2] The questions given him became continually harder. "What number is that which, being divided by the product of its digits, the quotient is 3; and if 18 be added the digits will be inverted? He flew out of his chair, whirled around, rolled up his eyes and said in about a minute, 24." "What is the entire surface of a regular pyramid whose slant height is 17 feet and the base a pentagon, of which each side is 33.5 feet? In about two minutes after amplifying round the room, as his custom is, he replied 3354.5558. 'How did you do it,' said I. He answered: Multiply 33.5 by 5 and that product by 8.5 and add this product to the product obtained by squaring 33.5, and multiplying the square by the tabular area taken from the table corresponding to a pentagon."

"Multiply in your head 365,365,365,365,365 by 365,365,365,365,365,365. He flew around the room like a top, pulled his pantaloons over the top of his boots, bit his hand, rolled his eyes in their sockets, sometimes smiling and talking, and then seeming to be in agony, until, in not more than one minute, said he, 133,491,850,208,566,925,016,658,299,941,-

[1] Chamber's Journal, VIII, p. 265.
[2] Chamber's Journal, VIII, p. 266.

583,225! . . . he began to multiply at the left hand and to bring out the answer from left to right."[1]

In the number of figures this exceeds Bidder's longest multiplication, but the repetition of the same figures renders it easier.

Safford had not a one-sided mind; "chemistry, botany, philosophy, geography and history are his sport." "His memory too is very retentive. He has pored over Gregory's Dictionary of the Arts and Sciences so much that I seriously doubt whether there can be a question asked him drawn from either of those immense volumes that he will not answer instantly." This reminds one of the story of Ampère and the encyclopedia.[2]

On an invitation of the Harvard University his father removed to Cambridge and Safford was placed under the charge of Principal Everett and Professor Peirce. At the age of 14 he calculated the elliptic elements of the first comet of 1849. After graduating from Harvard in 1854, he spent several years there in the observatory. Since this time he has made many important astronomical calculations and discoveries, and numerous contributions to the astronomical journals. He is at present Professor of Astronomy in Williams College.

In regard to the divisors of large numbers, Safford seemed to possess the power of recognizing in a few moments what numbers were likely to divide any given large number, and then of testing the matter by actual division with great rapidity.[3]

Miscellaneous.—A boy from St. Poelten was exhibited by Gall in Vienna. He was the son of a blacksmith and had received no more instruction at school than his companions. At nine years of age, when they gave him three numbers each expressed by ten or twelve figures, asking him to add them, then to subtract them two by two, to multiply and then

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[1] Chamber's Journal, VIII, p. 266.

[2] See Arago's Eulogy on Ampère, translated in the Smithsonian Report, 1872.

[3] Belgravia, XXXVIII, p. 456.

divide them by numbers containing three figures, he would give one look at the numbers and announce the result before it could be obtained by others on paper.[1]

Gall says that an advocate came to him to complain that his son, aged five years, was occupied exclusively with numbers and calculations, and that it was impossible to fix his attention on anything else.[2]

Devaux, a boy of seven years, had a passion for going to all the fairs, and waiting for the traders at the moment when they had closed their accounts; when they made mistakes in their calculations, it was his greatest pleasure to discover the error.[3]

Mr. Van R. of Utica, U. S. A., at the age of six years distinguished himself by a singular facility for calculating in his head; at eight he entirely lost this faculty, and after that time he could calculate neither better nor faster than any other person. He did not retain the slightest idea of the manner in which he performed his calculations in childhood.[4]

The daughter of Lord Mansfield, seen by Spurzheim at London, when she was 13 years old, almost equaled Colburn; she extracted with great facility the square and cube root of numbers of nine places.[5]

Prof. Elliot tells of a half idiot who was remarkable in his own county district for his powers of calculation. He got him to put down his operations in a few cases on paper; his modes of abbreviation were ingenious.[6]

Huber tells of a blind Swiss who solved the most difficult arithmetical problems, and who was able to repeat in either way a line of 150 figures after hearing them only once.[7]

II.

The duty of a psychological analysis of the powers of arithmetical prodigies would be to determine the processes of

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[1] Huber; Das Gedächtniss, München, 1878, p. 43.
[2] Gall, Functions of the Brain, Organology, XVIII.
[3] Gall, loc. cit.
[4] Gall, loc. cit.
[5] Medical and Philosophical Journal and Review, New York, 1811, p. 22.
[6] Gall. loc. cit.
[7] Spectator, 1878, LI, p. 1634.

which such powers consist and to establish a series of gradations from the normal to the abnormal. It lies, however, outside of our present task to investigate the fundamental arithmetical processes, though just these cases seem to offer a means of clearing up some of the obscurity; we shall not go beyond facts such as, accuracy of memory, arithmetical association, etc., which for our purposes can be regarded as not requiring further analysis.

Speaking of the ability to reckon rapidly, Gauss remarks: "Two things must be distinguished here, a powerful memory for figures and a real ability for calculation. These are really two qualities entirely independent of each other, which can be united but are not always so."[1] Bidder's opinion was "that mental calculation depends on two faculties of the mind in simultaneous operation—computing and registering the result."[2] Nevertheless, there are some other important facts in the psychology of the ready reckoners; we shall accordingly consider them in respect to memory, arithmetical association, inclination to mathematics, precocity and imagination.

MEMORY. Perhaps aside from prococity the most remarkable fact in regard to ready reckoners is their power to do long calculations wholly in the mind without making a mistake; next to this would be placed the wonderful rapidity which some of them have shown.

Accuracy of Memory.—The performance of long calculations in the mind depends above all on the accuracy of the memory for a sufficient length of time. For longer periods of time there seems considerable variation among the several calculators, and indeed this power is not an absolute necessity.

Buxton had perhaps the most accurate memory of all. For example, he gave from memory an account of all the ale or strong beer that he had on free cost since he was 12 years of age; this list included 57 different persons and 2130

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[1] Briefwechsel zwischen Gauss und Schumacher, V, 300.
[2] Proceedings, Civ. Eng., XV, 262.

glasses. "He will leave a long question half wrought and at the end of several months resume it, beginning where he left off, and proceeding regularly till it is completed."[2] Buxton was very slow and clumsy, but extremely accurate in his calculations, a fact which shows that his powers depended on an accurate memory.

Much the same is related of Fuller. "Though interrupted in the progress of his calculation and engaged in discourse upon any other subject, his operations were not thereby in the least deranged so as to make it necessary for him to begin again, but he would go on from where he had left off, and could give any or all of the stages through which the calculation had passed."[3]

Of Dase it is related that, "after spending half an hour on fresh questions, if asked to repeat the figures he began with, and what he had done with them, he would go over the whole correctly."[4] Half an hour after using the two numbers mentioned on p. 45, it was asked if he remembered them. "He instantly repeated the two numbers together (as a number containing 25 figures) forwards and backwards; 9 quadrillion, 351 thousand, 738 billions, etc."[5]

Of Colburn we have no account that represents him as having a good memory for a long time, yet he, as well as all the others, must have possessed extensive multiplication tables stored up indelibly in their minds. This is not to be confused with what we ordinarily call accuracy of memory, by which we mean that a thing or a number once seen is always retained. We may, however, extend the term and speak of acquired accuracy, where the retention results from a proper impression on the mind by means of association and repetition. Bidder, and probably several of the others, possessed wonderful memories, especially for figures; the acquisition of such a memory was due to their peculiar training,

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[1] Gent. Mag. XXIII, 557.
[2] Gent. Mag. XXIV, 251.
[3] Columbian Centinel, Dec. 29, 1790, No. 31 of Vol. XIV.
[4] Littell's Living Age, LIV, 1857, p. 62.
[5] Briefwechsel zw. Gauss und Schumacher, V. 302. The notation follows the continental system; in English it would be 9 octillions, 351 septillions, 738 sextillions, etc.

and, we suspect, to a lack of the ordinary mind-killing processes found in our schools. Bidder says: "As regards memory I had in boyhood, at school and at college many opportunities of comparing my powers of memory with those of others, and I am convinced that I do not possess that faculty in a remarkable degree.[1] If, however, I have not any extraordinary amount of memory I admit that my mind has received a degree of cultivation in dealing with figures in a particular manner which has induced in it a peculiar power; I repeat, however, that this power is, I believe, capable of being attained by any one disposed to devote to it the necessary time and attention."[2]

Although an accurate memory for a long time may not be possessed by every rapid calculator, he must be able to retain before the mind with absolute accuracy the results of the various processes performed till he has finished the problem. This we can pre-suppose in the case of every one of the arithmetical prodigies, and indeed it seems to have been the one thing in which Buxton was superior to ordinary mortals.

One secret of such an accurate memory while performing a calculation, lies in relieving it of unnecessary burdens. It will be noticed that the ready-reckoners often divided a multiplier into two factors and multiplied first by one and then the other; *e. g.*, 432×56 would be $432 \times 8=3456$; 432 and 8 can be now forgotten and $3456 \times 7=24192$; whereas in the ordinary way $432 \times 6=2592$, must be held in memory, while $432 \times 50=21600$ is performed, in order that the partial products may be added together.

There are other means used to lighten the work of the memory. Every one of those about whom we know anything in this respect gave his answers and probably did his work from left to right.[3] Colburn's explanation shows how he began with the highest denominations: "the large numbers found first are easily retained because consisting of so many ciphers."[4]

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[1] Proceedings, Civ. Eng. XV, 253.
[2] Proceedings, Civ. Eng. XV, 253.
[3] Memoirs, p. 191.
[4] See Memoirs, p. 189, 190.

Bidder explains why beginning at the left is easier and necessary. "I could neither remember the figures [in the ordinary way of multiplying], nor could I, unless by a great effort, on a particular occasion, recollect a series of lines of figures; but in mental arithmetic you begin at the left hand extremity, and you conclude at the unit, allowing only one fact to be impressed on the mind at a time. You modify that fact every instant as the process goes on; but still the object is to have one fact and one fact only, stored away at one time."[1] In doing the example 373 × 279, "I multiply 200 in 300=60,000; then multiplying 200 into 70, gives 14,000. I then add them together, and obliterating the previous figures from my mind, carry forward 74,600," etc.

"For instance, multiplying 173 × 397, the following process is performed mentally:

100 × 397=39,700
70 × 300=21,000=60,700
70 × 90 . = 6,300=67,000
70 × 7 . . = 490=67,490
3 × 300 . . . = 900=68,390
3 × 90 . . . . = 270=68,660
3 × 7 . . . . . = 21=68,681.

The last result in each operation being alone registered by the memory, all the previous results being consecutively obliterated until a total product is obtained."[2]

In trying to follow the method used by these men we are hampered by our inability to keep the hundreds, thousands, etc., in their proper places. When a person asks you suddenly how many figures in a million, can you answer him instantly? In his instruction for a ready computer De Morgan gives the following rule: "In numeration learn to connect each primary decimal number, 10; 100; 1000, etc., not with the place in which the unit falls, but with the number of ciphers following. Call ten a *one-cipher* number; a hundred a *two-cipher* number; a million a *six-cipher*, and so on."[3]

Various other little helps were used. Bidder reveals some of them: *e. g.*, "in questions involving division of time, distances, weight, money, etc., it is convenient to bear in mind

[1] Proceedings, Civ. Eng. XV, 260.
[2] Proceedings, Civ. Eng. XV, 260.
[3] De Morgan, Elements of Arithmetic, London, 1857, p. 161.

the number of seconds in a year, inches or barley-corns in a mile, ounces and pounds in a cwt. and ton, pence and farthings in a pound sterling, etc. . . . These were always ready for use when they could be applied with advantage. . . . Suppose it is required to find the number of barley-corns in 587 miles, the ordinary process, viz.: $1,760 \times 587 \times 3 \times 12 \times 3 = 111,576,960$, when worked out, requires 56 figures; while, mentally, I should multiply 190,080, the number of barley-corns in a mile, by 587."[1] When we consider that certain stock questions continually recur among those answered by the prodigies, the assistance of such facts is apparent. Safford always remembered the divisors of any number he had examined.[2]

Extraordinary as their powers were these men are not the only ones distinguished for remembering numbers. After a whole day's public sale, Hortensius could tell from memory all the things sold and their prices. Niebuhr could dictate a whole column of statistics from memory.[3] It is related that Alex. Gwin at 8 years of age knew the logarithms of all numbers from 1 to 1000. He could repeat them in regular order or otherwise.[4]

Of Dirichlet it is said that he possessed an "extraordinary power of memory, by means of which he had at every moment completely before him what he had previously thought and worked out."[5]

Euler had a prodigious memory for everything; this gave him the power of performing long mathematical operations in his head. While instructing his children, the extraction of roots obliged him to give them numbers which were squares; these he reckoned out in his head. Troubled by insomnia, one night he calculated the first six powers of all

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[1] Proc. Civ. Eng., XV, 266.
[2] Belgravia, XXXVIII, 456.
[3] Lieber, Reminiscences of Niebuhr, Phila. 1835, p. 46; also, Lieber's Miscellaneous Writings, Phila. 1881, I, 74.
[4] Belgravia, XXXVIII, 462.
[5] Kummer, Gedächtnissrede auf Gustav Peter Lejeune-Dirichlet, Separatabdr. aus dem Abhandl. d. Kgl. Akad. d. Wiss. zu Berlin, 1860, p. 34.

the numbers under 20, and recited them several days afterwards.[1]

There is on record the case of Daniel McCartney, born 1817, near Mt. Pleasant, Westmoreland Co., Penn., as late as 1871 living in Salem, Columbiana Co., Ohio, who was examined in 1870 by W. D. Henkle, State Commissioner of Public Schools in Ohio. The man showed a remarkable memory. Among other questions put to him were the following which indicate a power not so great as Buxton's but yet remarkable: "Ques.—What is 123 times 456? Ans. (35 seconds), 56,088. Multiply 456 by 100; then 23 by 400; then add; multiply 23 by 56 and add. Ques.—What is 3756 times 182? Ans. (4½ minutes. He became confused), 683,-592. Ques.—What is the sum of 26, 67, 43, 38, 54, 62, 87, 65, 53, 44, 77, 33, 84, 56 and 14? (One minute occupied in calling the numbers.) Ans. (Instantly) 803."[2]

Still more remarkable is the case of Wallis, the mathematician. In a letter to Thomas Smith of Madalene College, Wallis tells his own story:

"December 22d, 1669.—In a dark night, in bed, without pen, ink or paper or anything equivalent, I did by memory extract the square root of 30000,00000,00000,00000,00000,-00000,00000,00000, which I found to be 1,77205,08075,68077,-29353, *ferè*, and did the next day commit it to writing."

"February 18th, 1670.—Joannes Georgius Pelshower (Regimontanus Borussus) giving me a visit, and desiring an example of the like, I did that night propose to myself in the dark without help to my memory a number in 53 places: 24681357910111214111315161820171921222426283023252729 31 of which I extracted the square root in 27 places: 15710301-687148280581715 2171 *proximè;* which numbers I did not commit to paper till he gave me another visit, March following, when I did from memory dictate them to him.

Yours, etc., JOHN WALLIS."[3]

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[1] Euleri commentationes arithmeticae collectae, Petropoli, 1849; tomus I, Éloge de L. Euler par N. Fuss, p. XLIX; see also Condorcet's eulogy of Euler.

[2] Remarkable Cases of Memory, in the Journal of Speculative Philosophy, 1871, Vol. V, p. 16.

[3] A copy of this letter is to be found in the Spectator, 1879, Vol. LII, p. 11.

We have here selected a series beginning with Hortensius and Niebuhr, who simply remembered numbers, and proceeding to men who used their memories in calculating with as much success as Buxton. None of these men could well be placed among the arithmetical prodigies, yet Buxton seems to have differed from McCartney only in his interest for figures, whereas in Euler and Wallis the calculating power was lost sight of. Like these men, Buxton showed none of the rapidity seen in all the other calculators.

Performing long calculations in the head has been compared to blindfold chess-playing. When rapidity is left out of consideration, as in Buxton's case, the same power of memory may perhaps account for both. Indeed, Geo. Bidder, Q. C., who possessed a strong power of visual imagery, is able to play two simultaneous games of chess without seeing the board.

Rapidity of Memory.—The rapidity of a memory will depend on the nature of the various processes of the mind which make up the phenomenon called by that name and also on the rapidity with which these processes work. Our power to rapidly commit a group of objects or a line of a dozen figures to memory and to call it up again instantly, depends on the ease and rapidity with which we can impress it on the mind, on the accuracy with which it is retained and the ease and rapidity with which it can be reproduced. The accuracy of retention, being of course only a manifestation of the accuracy of memory, has already been considered.

The ease and rapidity with which a number of objects can be impressed on the memory seem limited in ordinary persons to about five at a glance. Before the days of experimental psychology this was quite a matter of dispute,[1] but it has been in late years definitely settled. The first experiments seem to have been made by Stanley Jevons, who decides that his power does not reach to five with complete accuracy, and that the error in estimating numbers under such conditions, $= \frac{n}{9} - \frac{1}{2}$, where n is the number of objects.[2]

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[1] Some of the opinions are given in Hamilton's Lectures on Metaphysics, Vol. I, 253.

[2] The Power of Numerical Discrimination, in Nature, Vol. III, 281.

Preyer has made some popular experiments, from which he concludes that after practice a person can estimate in general correctly up to nine objects seen for an instant, when these objects are irregularly grouped, and that acquaintance with a symmetric arrangement, as in cards or dominoes, raises the limit to about 40.[1] Cattell also made experiments on the extent of the focus of consciousness, which show that 4 to 5 unconnected impressions (lines, letters, figures) can be simultaneously apperceived. When these elements were placed in well known groups the number rose to 12 and 15.[2]

There are, however, two processes to be distinguished, the perception of the objects and the counting of them. Cattell's experiments show how many can be distinctly apperceived; but the power of counting them may depend on the maintenance of the apperceived and even the perceived objects in the memory for a sufficient time. Wishing to know how it is possible to count a number of objects seen for so brief a time, I exposed a few objects to the view for an instant; the person observing had then to tell how many objects were seen. One of the observers gave the first number thought of without being able to tell why; the other always counted or attempted to count the objects from a picture of them which he held in his memory.

All the arithmetical prodigies possessed a remarkable impressibility; they were able to grasp large numbers of figures on only once seeing or hearing them. Dase, morever, has given special proofs of his power by his experiments in rapid counting. "When you throw a handful of peas on the table, a casual glance is sufficient to enable him to tell you their number. He did the same . . . with the points of dominoes at which he gave only a momentary glance in order to tell their sum (117)."[3] "He counted the letters in a line on an octavo and a quarto page (47 and 63) after a hasty glance."[4] Dase's memory also possessed great impressibility

[1] Counting Unconsciously, in Pop. Science Monthly, XXIX, p. 221.

[2] Cattell, Psychometrische Untersuchungen, in Philos. Studien, III, 121; Wundt, Phys. Psychol. 3 Aufl. II, 247.

[3] Briefwechsel zw. Gauss und Schumacher, V, 277.

[4] Briefwechsel zw. Gauss und Schumacher, V, 302.

for figures. "Twelve figures being written down . . . he would just dip his eye upon them, not resting on them more than half a second. He would then repeat them backwards and forwards, and name any one at command, as the ninth or the fourth."[1] Dase can be contrasted with ordinary individuals in this respect. The experiments referred to in the AMERICAN JOURNAL OF PSYCHOLOGY, Vol. II, 607, 608, show that the largest number of numerals that could be learned by once hearing them at the rate of 120 to the minute, was 8.6 for boys of 19 years. Even Mondeux required 5 minutes to learn and retain a number of 24 figures divided into 4 periods, in such a way that he could give at will the six figures in each period.

Such quick apprehension of a number as Dase's can be explained by great impressibility; in which case the visual image would be in such a short time so firmly and vividly impressed on the memory that he could turn away his eyes and count the peas or domino points from a still persistent image, just as the person mentioned above did. The case would then be exactly like that of Robert Houdin and his son. They would pass rapidly before a toy shop and cast an attentive glance upon it; a few steps further on they tried which could describe the greatest number of objects seen. The son often reached 40, the father 30. An instance is also given in which the son saw at a glance and remembered the titles of many books of a library. This power of memory was not a natural gift. Houdin taught his son by laying dominoes before him; instead of letting him count the points the boy had to tell the total at once. In three days he could count six dominoes (from 15 to 61 points) and in a short time he could give instantaneously the sum of a dozen (up to 106 points.) In like manner it is possible to learn to commit a row of figures to memory in an instant. "A useful faculty, easily developed by practice, is that of retaining a retinal picture. A scene is flashed upon the eye: the memory of it persists, and details, . . . may be studied . . . in a subsequent vision."[2]

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[1] Littell's Liv. Age, 1857, LIV, p. 62.
[2] Galton, Inquiries into Human Faculty, London, 1883; p. 107.

ARITHMETICAL ASSOCIATION. The psychology of calculation is still an unexplored field; yet for our purpose we can regard the association of numbers as elementary, leaving further analysis for future investigation. The process is that which was taught us in school; we learned to say 1 and 1 make 2, 1 and 2 make 3, etc., 1 less 1 leaves 0, 2 less 1 leaves 1, etc., $1 \times 1 = 1$, $1 \times 2 = 2$, etc.; 1 divided by 1=1, 2 divided by 1=2, and so on through the rest of the tables. By this means firm associations are gradually established between any two numbers up to 10 (in older boys often to 12 and 15) in all of the four relations. After thoroughly learning these associations we are able to "do sums." Suppose we had this example to solve: What is the sum of 2571 and 4249? The process we go through is—when we write in the order we do it—as follows:

9 and 1 = 10, put down 0, carry 1.
4 and 7 = 11, and 1 = 12, put down 2, carry 1.
2 and 5 = 7, and 1 = 8.
4 and 2 = 6.
total, 6 thousand, 8 hundred and 20.

Or take an example in multiplication, *e. g.*, 136 by 43. What do you say to yourself while working it?

3 times 6 are 18, put down 8, carry 1;
3 times 3 are 9, and 1 are 10, put down 0, carry 1;
3 times 1 are 3, and 1 are 4;
total, 408.
4 times 6 are 24, put down 4, carry 2;
4 times 3 are 12, and 2 are 14, put down 4, carry 1;
4 times 1 are 4, and 1 are 5;
total 544;
8;
4 and 0 are 4;
4 and 4 are 8;
5;
result, 5848.

This is exactly the way in which children in school generally reckon, even when they have no distinct intention of reckoning aloud. I must also confess that although I long since left school, whenever my mind is tired or distracted I have to go through the same process and cannot put into practice the various methods of "cutting off" that have been since learned.

These "cut-offs" are found in all our activities, and consist in part of a train of thoughts or volitions becoming less

and less conscious. Movements which regularly follow certain sense-perceptions have the tendency to become automatic, and *to occupy less time*.[1] In like manner it has been shown that a series of ideas can be gone through, although one of them can sink below consciousness without destroying the sequence.[2] In this way our arithmetical associations can be enormously shortened. In the above example we shall totally disregard the time used in recording results mentally, and try only to shorten the associations. From my own experience I can say that in the first place for most of the associations I can reduce the connecting links between the numbers to an extremely small degree of consciousness, with a greater or less saving of time. Instead of saying "plus," "less," "by," etc., I simply repeat the numbers and the results, and although I know perfectly what I am doing, and make no confusion among addition, multiplication, etc., nevertheless these relations do not rise above a very low degree of consciousness: "9, 5, 14," "9, 5, 4," "9, 5, 45," are perfectly distinct and clear, yet I do not think consciously of any of the operations performed. In like manner various connecting links can be cut out; so that for instance, the example given above would for me be reduced to

3, 6, 18, 1, 8;[3]
3, 3, 9, 1, 10, 1, 0;
3, 1, 3, 1, 4;
408;
4, 6, 24, 2, 4;
4, 3, 12, 2, 14, 1, 4;
4, 1, 4, 1, 5;
544
5848[4]

"The act of addition must be made in the mind without assistance; you must not permit yourself to say 4 and 7 are 11, 11 and 7 are 18, etc."[5] "Learn the multiplication table

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[1] See Wundt, Phys. Psych. 3 Aufl. II, 319.
[2] Scripture, Ueber den associativen Verlauf der Vorstellungen; Inaug. Diss., Leipzig, 1891.
[3] In such cases I always imagine the number, *e. g.*, 18, and then take away the 1, leaving the 8, so that the "carrying" occurs before the "putting down."
[4] In mental examples I also add from top to bottom, and in easy cases from left to right.
[5] De Morgan, Elements of Arith., p. 162.

so well as to name the product the instant the factors are seen; that is, until 8 and 7, or 7 and 8 suggest 56 at once, without the necessity of saying, 7 times 8 are 56."[1] Of course the saving of time is very great; and yet an educated person can work with just as much, perhaps more accuracy, than in the unabbreviated style.

Still another shortening can be made; by making an effort I can do "the carrying unconsciously so that in the above case I would say,

3, 6, 18, 8,

and not think of the 1 until the time for adding it in occurs. As De Morgan remarks, "don't *say* 'carry 3' but *do* it." Moreover it is not absolutely necessary to distinctly mark the end figure of each partial product; these products can be kept in memory and added up afterwards. The above example would be carried out by a person who had good command of such a power in somewhat the following manner (the same figures denote that they were operated upon before they entered full consciousness:)

$^{3}, {}^{6}, 18;$
$^{3}, {}^{3}, 9, {}^{1}\ 10;$
$^{3}, {}^{1}, 3, {}^{1}, 4;$
$408;$
$^{4}, {}^{6}, 24;$
$^{4}, {}^{3}, 12, {}^{2}, 14;$
$^{4}, {}^{1}, 4, {}^{1}, 5;$
$544;$
$5848.$

This shortening can in adding be carried to such an extent that only the results are noticed; *e. g.*, as soon as a person catches a glimpse of 405 and 540 he knows the sum. In the above case the time for associating the numbers with their products has become exceedingly small; as Bidder says most of the time is required for the registration of the results on the memory, and this as was shown above can in exceptional cases be very small.

Finally an enormous shortening can be made if the adding, subtracting, multiplying, etc., can be done *before the numbers themselves come into full consciousness.* Münsterberg has shown that associations made in such a low degree of

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[1] Ibid, p. 163.

consciousness require comparatively little time.[1] A few years ago I made the attempt to acquire this ability and after considerable practice I was able on the sight of two figures to add or subtract them before they had attracted my full attention; in other words while they were yet in the field of consciousness they aroused the proper association and the result entered the focus of consciousness first.

We might be tempted to carry the process of "cutting-off" consciousness still further, and to say in just the same manner as on the sight of the figures 136×43 the partial products spring at once of themselves into the mind of a mathematician so in exceptional cases these partial products might be added before they became fully conscious, so that nothing but the result appears; a further application of the "cut-off" would bring the final answer to the whole problem instantly into mind. To be sure the testimony of the elder Bidder is against this, but it is only an extension of the principle and seems necessary to explain the difficulty of Colburn in telling how he did his examples. In his early years all he could say was that the problem was given and the answer was almost at once there. It would also help to explain such cases as are furnished by Dase; he had been given the number

935173853927;

Schumacher mentioned 7. "As soon as he heard 7 he repeated the number

6546216977489."[2]

This performance of processes before the factors became fully conscious would show itself in the popping of the answer into the mind before the person has thought out clearly how it was obtained. Upon the involuntary answers the rapid calculator would have to rely; if he stopped to make sure of each step time would be lost; he must always go ahead without a question as to whether he is right or not. The younger Bidder says, "I am certain that unhesitating confidence is half the battle. In mental arithmetic it is most true that he who hesitates is lost."[3]

[1] Münsterberg, Beiträge zur experimentalen Psychologie, Freiburg $^1/_B$ 1890, Heft 1.
[2] Briefwechsel zw. Gauss und Schumacher, V 302.
[3] Spectator, 1878, p. 1634.

In still another way it would be possible to save time. It is common practice to extend the multiplication table beyond ten at least to 12×12. Here two figures are multiplied by two figures. In like manner it is easy to learn the table of 15 or 25 and with an effort we could undoubtedly learn complete tables of addition, subtraction, multiplication, division up to perhaps 30.[1] In an example like the following we would divide the number into periods of two figures each and operate directly with them:

2419 × 3017
19, 17 323
24, 17 408
30, 17 570
30, 24 720

7,298,123

To get an idea of the wonderful ease and rapidity with which examples can be done in this way make use of a multiplication table reaching to 100×100.[2] When moreover no time is lost in turning the leaves of such a table, in running down a column and recording the results on paper, then a person who could hold such a table in his head ought to be able to answer many problems in less time than even Dase required.

It is really not so difficult to obtain such a table of the products of two figures. "I formerly knew an instructor whose scholars of 8 to 12 years of age, for the most part knew the Pythagorean table extended to 100×100, and who calculated rapidly in the head the products of two numbers of four figures, in making the multiplication by periods of two figures."[3] Did any of the prodigies possess such a table? Considering their enormous powers of memory it would be almost unexplainable if they did not. Although Bidder asserts that he really had no such table, yet Mondeux actually possessed part of such a table, and I think we can pre-suppose it in the case of Colburn, Buxton and even Dase.

[1] "In my opinion, all pupils who show a tolerable capacity should slowly commit the products to memory as far as 20 times 20." DeMorgan, Elements of Arithmetic, London, 1857, p. 25.

[2] For example, Waldo's Multiplication and Division Table for Accountants, Computers and Teachers in the Primary Schools; New York, 1880.

[3] Lucas, Le calcul et les maschines à calculer, Assoc. française pour l'avancement des sciences, Paris, 1884, p. 2.

A number of other rules by which the processes of addition, subtraction, etc., can be shortened are given by DeMorgan in his Elements of Arithmetic, Appendix I; also in Companion to the Almanac 1844 and Supplement to the Penny Cyclopædia, article Computation.

There are also little "kinks" put in practice by many people of which the ready reckoners were not slow to avail themselves, *e. g.*, multiplying by two easy numbers and taking the difference instead of multiplying by an awkward number. In regard to the example given above p. 36 Bidder says "I should know at a glance, that

$$
\begin{array}{lrl}
 & 400 \times 173 = & 69{,}200 \\
\text{and then} & 3 \times 173 = & 519 \\
\hline
\text{the difference being} & & 68{,}681.\text{"}
\end{array}
$$
[1]

Now that we have some idea of how the mind works in solving arithmetical problems, and of how it shortens the time required, let us see how the prodigies actually worked.

Dase, on a test before Schumacher, divided "each number into two parts, of which one contains the highest figures and three zeros, and the other the three lower figures, reckoned the 4 partial products in his head, and *noted down every time* separately the results with pencil, which he afterwards added mentally."[2] As Gauss said, Dase seems to have depended on his remarkable accuracy of memory and to have possessed powers of calculation which at best were not equal to those of many mathematicians. "When he needs $8\frac{3}{4}$ hours to multiply two numbers each of 100 figures in his head, this is really a foolish waste of time, for a moderately practised reckoner could do the same on paper in *much* shorter, in less than half the time."[3] Gauss, however, was himself such a wonderful reckoner that judging from the standpoint of an ordinary person, he underestimated Dase's powers.

Buxton was much slower, as is seen from the following: "Admit a field 423 yards long and 383 wide, what was the area? After I had read the figures to him distinctly, he gave me the true product, viz., 162009 yards, in two minutes, for

[1] Proceedings Civ. Eng. XV 260.
[2] Briefwechsel zw. Gauss und Schumacher, V, 32.
[3] Briefwechsel zw. Gauss und Schumacher, V, 300.

I observed by my watch how long every operation took him."[1] On paper this is easily done in twenty seconds. "Allowing the distance between York and London to be 204 miles, I asked him how many times a coach-wheel turned round in that distance, allowing the wheel's circumference to be six yards! In 13 minutes he answered 59,840 times."[2] On paper this requires 35 seconds. The clumsiness of Buxton's methods is phenomenal. He was required to multiply 456×378. This he did as follows:

$$
\begin{aligned}
&456 \times 5 = 2280, \text{ which } \times 20 = 45600;\\
&45600 \times 3 = 136800.\\
&2280 \times 15 = 34200;\\
&136800 + 34200 = 171000;\\
&456 \times 3 = 1368;\\
&171000 + 1368 = 172368.
\end{aligned}
$$
[3]

When Mondeux had to multiply two entire numbers he often divided them into portions of two figures; he recognized that in a case where the factors are equal the operation is simpler, and the rules used by him for obtaining the product, or rather the power demanded are precisely those given by Newton's binomial formula.[4] That is to say, in an example, 2419×3017, he would proceed as we have done above, and in a case like 2419×2419 (or 2419^2) or 2419 to any power he worked according to the formulas $x^2 + 2\,xy + y^2$ (*i. e.*, $24^2 \times [2 \times 24 \times 19] + 19^2$), $x^3 + 3\,x^2\,y + 3\,xy^2 + y^3$, etc. "Guided by these rules he could give on the instant the squares and cubes of a multitude of numbers; for example, the square of 1204 or the cube of 1006. As he knows almost by heart the squares of all the entire numbers less than 100, the division of the greater numbers into periods of two figures enables him to obtain their squares more easily."[5]

A partial account of Bidder's method's of multiplication has already been given; here it is necessary only to add a few facts left untouched and an explanation of his ways of extracting roots and finding factors. Most important is the contrast between his multiplication table, understood and

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1 Gent. Mag. XXI, 347.
2 Gent. Mag. XXI, 347.
3 Gent. Mag. XXIV, 251.
4 Comptes rendus, XI, 953.
5 Comptes rendus, XI, 954.

made part of himself, and the mechanical associations of most people.

"In order to multiply up to 3 places of figures by 3 figures, the number of facts I had to store in my mind was less than what was requisite for the acquisition of the common multiplication table up to 12 times 12. For the latter it is necessary to retain 72 facts; whereas, my multiplication up to 10 times 10 required only 50 facts. Then I had only to recollect, in addition, the permutations among the numbers up to a million, that is to say, I had to recollect that 100 times 100 were 10,000; 10 times 10,000 were 100,000, and that ten hundred thousands made a million. . . . . Therefore, all the machinery requisite to multiply up to 3 places of figures was restricted to 68 facts. . . . . If you ask a boy abruptly, "what is 900 times 80," he hesitates and cannot answer, because the permutations are not apparent to him; but if he had the required facts as much at his command as he had any fact in the ordinary multiplication table, viz., that 10 times 10 = 100, and that 900 times 80 was nothing more than 9 times 8 by 100 times 10, he would answer off hand 72,000; and if he could answer that, he would easily say 900 times 800 = 720,000. If the facts were stored away in his mind so as to be available at the instant he would give the answer without hesitation. If a boy had that power at his command he might at once with an ordinary memory proceed to compute and calculate 3 places of figures."[1]

The following gives an insight into the rapidity of Bidder's associations: "Suppose I had to multiply 89 by 73, I should say instantly 6,497; if I read the figures written out before me I could not express a result more correctly or more rapidly; this facility has, however, tended to deceive me, for I fancied that I possessed a multiplication table up to 100 times 100, and when in full practice even beyond that; but I was in error; the fact is that I go through the entire operation of the computation in that short interval of time which it takes me to announce the result." The velocity of the

[1] Proceedings Civ. Eng., XV, 259.

mental processes cannot be adequately expressed; the utterance of words cannot equal it. . . . . Were my powers of registration at all equal to the powers of reasoning or execution, I should have no difficulty in an inconceivably short space of time in composing a voluminous table of logarithms."[1]

The least intelligible of all the explanations given by ready reckoners is that of Colburn. His friends tried to elicit a disclosure of the methods by which he performed his calculations, but for nearly three years he was unable to satisfy their inquiries. He positively declared that he did not know how the answers came into his mind.[2] In London he made a couple of explanations. "In one case he was asked to tell the square of 4395: he at first hesitated, ... but when he applied himself to it he said it was 19,316,025. On being questioned as to the cause of his hesitation, he replied that he did not like to multiply four figures by four figures; but, said he, I found out another way: I multiplied 293 by 293, and then multiplied this product twice by the number 15 which produced the same result. On another occasion, when asked the product of 21,734 multiplied by 543, he immediately replied, 11,801,562; but, upon some remark being made on the subject, the child said that he had, in his own mind, multiplied 65,202 by 181 [$21734 \times (181 \times 3) = (21734 \times 3) \times 181$]."[3]

Finally, it is worthy of remark that the attempt has been made to teach the performance of long multiplications without writing more than the problem and the answer. Although the method proposed is undoubtedly not the best, yet it suggests the possibility of inventing a practicable school-method. For example, multiply in one line 2681475 by 93165. Number the figures with indices as shown:

$$\begin{array}{r} {}^{7\,6\,5\,4\,3\,2\,1} \\ 2681475 \\ {}^{4\,3\,2\,1\,0} \\ 93165 \\ \hline {}^{11\,10\,9\,8\,7\,6\,5\,4\,3\,2\,1} \\ 249819618375 \end{array}$$

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[1] Proceedings Civ. Eng. XV, 255.
[2] Memoirs, p. 39.
[3] Analectic Mag. I, 1813.

Place the product of any two figures in that place of the result which has an index equal to the sum of the indices, of course adding in any carried numbers. Thus, $\overset{0}{5} \times \overset{1}{5} = 25$; the sum of their indices being 1 the 5 goes in the first place. $(\overset{0}{5} \times \overset{2}{7}) + (\overset{1}{6} \times \overset{1}{5}) + 2 = 67$; the 7 goes in the second place. $(\overset{0}{5} \times \overset{3}{4}) + (\overset{1}{6} \times \overset{2}{7}) + (\overset{2}{1} \times \overset{1}{5}) + 6 = 73$; the 3 goes in the 3rd. place, etc. Such a method of multiplication would undoubtedly be of assistance in training the ability for mental calculation. Of course we do not advocate an attempt at doing such enormous problems wholly in the mind, but shorter ones can be easily learned to great advantage. We should, however, take a few hints from Bidder, Safford, Colburn, *et al.* Suppose we had 379 × 42. Let us mentally index the numbers as above; then 4 × 3 = 12, which belongs

$$\begin{array}{r} 321 \\ 379 \\ 10 \\ \underline{42} \\ 4321 \\ 12 \\ 126 \\ 154 \\ 1554 \\ 1590 \\ 15918 \end{array}$$

in the 4th place. $3 \times 2 = \overset{3}{6}$; add this on to the other, 126. $4 \times 7 = \overset{3}{28}$; add to 126=154. $7 \times 2 = \overset{2}{14}$; add on to 154=1554. $4 \times 9 = \overset{2}{36}$; add, = 1590. 9 × 2=18; add on, =15918, Ans.

Mentally the figures would not be repeated, but as Bidder explains, the first obtained result would be modified. As a help in learning to keep the correct places, a card with several numbered compartments might be placed before the eyes, at first actually, then mentally; thus,

8 7 6 5 4 3 2 1

For paper or slate this of course requires more time and figures; but mentally such a process is quite possible, whereas the ordinary way of multiplying 3 figures by 2 figures is absolutely impossible to an ordinary person. With practice boys of advanced classes could undoubtedly be taught to multiply even 3 figures by 3 figures in the head.

R. A. Proctor, using Colburn as an illustration, explains the feats of calculating boys by an increased power of picturing a number as so many things and of modifying this picture

according to the operation to be performed.[1] 24 would be presented as

If 24 were to be multiplied by three all that is necessary is to picture three sets of dots; then to conceive the imperfect columns brought together on the right, giving six columns of ten and three columns each of four dots; and these three give at once (by heaping them up properly) another column of ten with two over: in all seven columns of ten and one of two,—that is, seventy-two. Proctor, who remarks that "all good calculators have the power of picturing numbers not as represented by such and such digits, but as composed of so many things,"[2] and who once possessed this power in no inconsiderable degree, says of this example that it "takes long in writing, but as pictured in the mind's eye, the three sets representing 24 formed themselves into the single set representing 72 in the twinkling of an eye."[3]

The suggestion is ingenious but it is only a suggestion. Unfortunately for Proctor's attempt to explain how the ready-reckoners reckoned, several of them have given extended accounts of the processes employed by them. The appendices to Colburn's Memoirs (of which Proctor did not know, for he says, "if Colburn had retained his skill until he had acquired power to explain his method, etc.") give an account of his methods of multiplying, extracting roots, etc., which flatly contradicts Proctor's explanation. In regard to Bidder Proctor afterwards admits that there was no room to doubt that his processes of mental arithmetic were commonly only

[1] Cornhill Mag. XXXII, p. 163; Science Byways, p. 349.
[2] Belgravia, XXXVIII, p. 451.
[3] Cornhill Mag., XXXII, p. 163; Science Byways, p. 350.

modifications of the usual processes.[1] Proctor arrived at this opinion on the evidence furnished by persons who had known Bidder (Bidder's own account was unknown to him). It is quite a confirmation of the theory of rapid calculation I have proposed, to find that the explanation of Bidder's powers advanced by Proctor is contained as one of the parts of my explanation, which is founded on the first hand evidence of Colburn, Bidder, Dase, etc.

Among the other mathematical operations in which the prodigies distinguished themselves more or less is the extraction of roots of numbers. In the first place it is to be remarked that Buxton knew nothing of this operation, and on the one occasion on which such a problem was given him he succeeded only approximately after a long time, apparently by running over the squares of various numbers till he found the one nearest to the given square.[2] Dase liked to extract the 5th root, "because he had noticed that in the fifth power the units are the same as in the root. I saw that with our system of numbers the $(4\,n+1)$ power has the same units as the root, a rule of which his is only a single case (for $n=1$)"[3]

In an appendix to his Memoirs, Colburn attempts to explain his methods of finding square and cube roots and of factoring. His rule, first formulated two years after he began, was as follows: Find a number whose square ending with the last two figures of the given square; then, when the given square consists of five places, what number squared will come nearest under the first figure (when 6 places, then the first two figures, when 7, the first 3, etc.) of the given square. For example:

What is $\sqrt{92{,}416}$?
1. What number squared ends in 16? Ans. 04.
2. What number squared comes next to 9? Ans. 3.
   Square root, 304.

What is $\sqrt[3]{321{,}489}$?
1. What number cubed ends in 89? Ans. 67.
2. What number cubed comes nearest to 32? Ans. 5.
   Cube root, 567.

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[1] Belgravia, XXXVIII, p. 456.
[2] Gent. Mag. XXIII, p. 557.
[3] Briefwechsel zw. Gauss und Schumacher, V, 382.

Colburn gives a table of the numbers which squared produce any given termination; to each termination there are four possible roots (to 25 there are 10) from which he must choose; *e. g.* a number ending in 16 can have one of the roots 04, 54, 46, or 96. "It is obvious that it requires a good share of quickness and discernment, in a large sum, to see which of the four roots . . . . is the right one."[1] The table for cube roots is very much simpler. These methods are of use only when the given number is an exact square or cube. Both depend on the last two figures, and a person would "probably greatly confuse the calculator by merely adding a small number to the square or cube."[2] Nothing ever excited so much surprise as the facility with which Bidder extracted square and cube roots. "Yet there is no part of mental calculation for which I am entitled to less credit. In fact, it is a mere slight of art." "Nearly every example proposed to me was a true square or cube; hence I hit upon the following expedient. . . . ."[3] He then gives a method exactly like that of Colburn.

It is not necessary to enter into the question how the prodigies found the factors of numbers. Colburn's process is found *in extenso*, on p. 183 of his Memoirs. It is clumsy and involved; he himself allowed it to be a "drag of a method."[4] Bidder's methods are explained on pages 272, 273 and 274 of Vol. XV of the Proc. Civ. Eng.

There is one other characteristic of the association of numbers that meets us in some of the persons under consideration, namely, the firmness with which long series of arithmetical associations cling together. This is seen in the independence of a process of reckoning among other activities and other processes of reckoning. Of Mondeux we read that his thoughts were as strongly directed to the arithmetical operation he had to perform as if he were completely isolated from his whole environment.[5] Buxton would talk freely whilst doing his questions, it being no molestation or

[1] Memoirs, p. 181.
[2] Hamilton's letter in Graves' Life of Sir Wm. R. Hamilton, p. 78.
[3] Proceedings Civ. Eng. XV, p. 266.
[4] Graves, Life of Sir Wm. R. Hamilton, p. 78.
[5] Comptes rendus, XI, p. 956.

hindrance to him.[1] "He would suffer two people to propose different questions, one immediately after another, and give each their respective answers without the least confusion."[2] In this not so very uncommon ability of doing two things at once the mathematicians seem to be specially favored. Dirichlet, for example, says "that he established the solution of one of the difficult problems of the theory of numbers, with which he had for a long time striven in vain, in the Sixtine Chapel in Rome while listening to the Easter music."[3]

MATHEMATICAL INCLINATION. The peculiar fascination for performing arithmetical calculations is sometimes a source of pleasure in itself; a distinguished savant during a public meeting undertook the multiplication of two long lines of figures and explained his action by "the pleasure it would give him to prove his calculation by division."[4] At the sight of figures, geometrical diagrams, and above all, algebraic formulas, young Galois was seized with a veritable passion for the abstract truths hidden behind these symbols.[5]

Even after Safford had lost his powers he continued to find pleasure in taking large numbers to pieces by dividing them into factors, or in satisfying himself that they were prime.[6] The younger Bidder remarks, "With my father as with myself the mental handling of numbers or playing with figures afforded a positive pleasure and constant occupation of leisure moments. Even up to the last year of his life my father took delight in working out long and difficult arithmetical and geometrical problems."[7]

In regard to special inclination to mathematics and its relation to ability for calculation, and also to other abilities, great diversity is shown by the persons we have considered. They can be variously grouped:

1. Those having strong mathematical inclinations with great powers of mental calculation (not necessarily rapid):

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[1] Gent. Mag. XXI, p. 347.
[2] Gent. Mag. XXI, p. 61.
[3] Kummer, Gedächtnissrede, etc., p. 34.
[4] Èloge d' Ampère, Smithsonian Report, 1872, p. 112.
[5] Magasin pittoresque, 1848, t. XVI, p. 227.
[6] Belgravia, XXXVIII, p. 456.
[7] Spectator, 1878, p. 1634.

here we should include nearly all arithmetical prodigies, although Colburn took no satisfaction in answering questions by the mere operation of mind; unless questioned, his attention was not engrossed by it at all; the study of arithmetic was not particularly interesting to him, but it afforded a very pleasing employment.[1] Nevertheless, the fascination for calculation was in some cases overpowering. Gauss considered mathematics the queen of the sciences and arithmetic the queen of mathematics; Buxton had neither eyes nor ears for anything else, and Mondeux and Dase greatly resembled him.

Corresponding to this class we might point out more than one distinguished mathematician who had not the ability to calculate; indeed, it would not be going too far to say that nine out of ten mathematicians have at least no liking for reckoning.

2. Those with inclination and ability for mathematics, including arithmetic: Nickomachos, Gauss, Ampère, Safford, Bidder.

3. Those with special inclination and ability for arithmetic alone;

a. having had no opportunities for other branches of mathematics: Fuller, Buxton, Mangiamele;

b. in spite of opportunities: Colburn, Dase, Mondeux.

c. where the talent disappears before opportunity for development is possible: Whately.

MATHEMATICAL PRECOCITY. "There are children, I know," says Arago, "whose apathy nothing seems able to arouse, and others again who take an interest in everything, amuse themselves with even mathematical calculations without an object." There are still others more seldom than either of these classes, who confine their interest to mathematical calculations alone. Strange as the fascination for arithmetic seems, it becomes still more so when it is manifested at an age at which it is normally absent; strangest of all is the union of ability to the inclination.

Hamilton included calculation in an all-sided precocity;[2]

[1] Memoirs, p. 69.
[2] Graves, Life of Sir Wm. R. Hamilton, Dublin.

Pascal's ability was for geometry,[1] as was also Clairaut's.[2] With Whately, Colburn, Bidder, Mondeux and Mangiamele the precocity showed itself alone in calculation; the same is true for Gauss's first years. Ampère and Safford, however, resemble Hamilton in showing inclination and ability for the most varied pursuits; the difference being that the mathematical side showed itself in Hamilton after the philological.

Special precocity in calculation showed itself (as far as our knowledge goes) at the following ages:

Gauss, 3,	Prolongeau, 6½,
Whately, 3,	Bidder, 10,
Ampère, between 3 and 5,	Mondeux, 10,
Safford, 6,	Mangiamele, 10,
Colburn, 6.	

It is remarkable that in nearly every case (possibly with the exception of Colburn and Whately) the arithmetical prodigies showed rather an extraordinary ability to learn calculation, not an ability to calculate before learning.

IMAGINATION. One peculiarity in the imaginative powers of the arithmetical prodigies is worthy of remark, namely their visual images. Bidder said, "If I perform a sum mentally it always proceeds in a visible form in my mind; indeed, I can conceive of no other way possible of doing mental arithmetic." This was a special case of his vivid imagination. He had the faculty of carrying about with him a vivid mental picture of the numbers, figures and diagrams with which he was occupied, so that he saw, as it were, on a slate the elements of the problem he was working. He had the capacity for seeing, as if photographed on his retina, the exact figures, whether arithmetical or geometrical, with which he was occupied at the time. This faculty was also inherited, but with a very remarkable difference. The younger Bidder thinks of each number in its own definite place in a number-form,[3] when, however, he is occupied in multiplying together two large numbers, his mind is so engrossed in the operation that the idea of locality in the series for the moment sinks out of prominence.[4] Is a number form injurious to calculating powers? The father seems to have arranged and used his

[1] Vie de Pascal par Mme. Perier.
[2] Nouv. Ann. de Math,, Paris, 1861, 1ière serie, XX, Bulletin, p. 50.
[3] Plate I, Fig. 20, in Galton's Inquiries into Human Faculty.
[4] Galton, Inquiries, etc., p. 134.

figures as he pleased; the son seems to be hindered by the tendency of the figures to take special places. It would be interesting to know if the grandchild, who possesses such a vivid imagination and in whom the calculating power is still further reduced, also possesses a number-form. The vivid, involuntary visualizing seems to indicate a lack of control over the imagination, which possibly entends to figures, and this perhaps makes the difference.

Colburn said that when making his calculations he saw them clearly before him.[1] It is said of Buxton that he preserved the several processes of multiplying the multiplicand by each figure of the lower line in their relative order, and place as on paper until the final product was found. From this it is reasonable to suppose that he preserved a mental image of the sum before him.

Of the other calculators we have no reports. Children in general do their mental problems in this way. Taine relates of one, that he saw the numbers he was working with as if they had been written on a slate.

The well-known case of Goethe's phantom, the case of Petrie, who works out sums by aid of an imaginary sliding rule, the chess-players who do not see the board, etc., are instances of the power of producing vivid visual imaginations that can be altered at will.

III.

Can we learn anything of practical use from the prodigies? The following points suggest themselves for consideration:

1. Bidder, Safford and the African brokers all speak for the fact that under cultivation the power of mental calculation could be greatly developed; the immense saving of time in school and afterwards that would result from an ability to shorten the associations, to use a multiplication table of two figures, and above all to register mentally, is sufficient to justify a trial.

2. Fuller, Ampère, Bidder, Mondeux, Buxton, Gauss, Whately, Colburn and Safford learned *numbers and their values* before figures, just as a child learns words and their meanings long before he can read. Bidder declares emphatically, "The reason for my obtaining the peculiar power of

[1] Med. and Philos. Journal and Review, New York, 1811, p. 22.

dealing with numbers may be attributed to the fact, that I understood the value of numbers before I knew the symbolical figures. . . . . In consequence of this, the numbers have always had a significance and a meaning to me very different to that which figures convey to children in general."[1]

3. Ampère, Bidder and Mondeux learned their arithmetic from pebbles. Arago says of Ampère, "It may be he had fallen upon the ingenious method of the Hindoos, or perhaps his pebbles were combined like the corn strung upon parallel lines by the Brahmin mathematicians of Pondichéry, Calcutta and Benares, and handled by them with such rapidity, precision and accuracy."

The Roman *abacus*, the Chinese *swanpan* and the success of the numeral-frames used in our primary schools, seem to point to the fact that it is best to teach "calculation" (*i. e.*, "pebbling" from Lat. *calculi*, pebbles), before "ciphering." The Arabic *tsaphara*, cipher, means empty; Arabic numeration, however, was considered mysterious by the people of the middle ages, and remains mysterious to many a child of to-day; to the former (and also not seldom to the latter) "ciphering" meant a secret and unintelligible process. If we could do away with the mystery of calculation perhaps the values of numbers and the tables might become then so indelibly fixed in the minds of children and so easy of application that they also could do long "sums" mentally or even carry the two-figure multiplication tables in their heads.

4. Dase's power of quick apprehension suggests the extension of the training sometimes attempted in schools, in which a slate with letters or figures is shown for an instant to the scholars who are then required to tell how much they recognized.

In conclusion it is necessary to express my obligations to President Hall and to Dr. de Perrot. To Dr. de Perrot of the Mathematical Department of this University, credit must be given for proposing the subject, for a large part of the references, and for numerous valuable suggestions and points of information.

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[1] Proceedings Civ. Eng. XV, 256.

THE PSYCHOLOGY OF TIME.

By Herbert Nichols.

III.—Experiments at Clark University.

In October, 1889, I was requested by the instructor in Psychology at Clark University to investigate the apparently contradictory results obtained by various experimenters regarding the Constant Error of Time-judgments. As a preliminary, the methods of previous experimenters were tested, until after several weeks, a single, and perhaps crucial point seemed to stand out as the proper question upon which to concentrate investigation, namely, the effect upon our estimation of any particular interval of previous sustained exercise or practice upon some other interval. A long series of experiments was then regularly undertaken which lasted several hours daily, for a period of over nine months of actual experimental work. 27 persons were tested; over 500 "sittings," or series of reproductions were made, comprising a total of approximately 50000 single judgments recorded. Five lengths of interval were chiefly used, namely: .25, .50, .75, 1.25, 1.75, seconds.[1]

Apparatus: After trying different metronomes in various ways, these were abandoned as inaccurate. Previous to beginning our regular experiments a nearly perfect instrument for beating time was found in a pendulum constructed as follows: A stiff bar, thin but wide, and five feet long, swung upon knife edges projecting from opposite sides a little above the middle of the length of the bar, and resting upon smooth metal plates, was supported by firm frame-work. Upon each end of the bar was a heavy 'bob' or weight which could be slid up or down and fastened with a spring and clamp-screw at any distance from the point of support. With the first pendulum made, any length of interval could

[1] As before, the unit throughout this section is one second, except where specifically stated to the contrary.

be obtained, by proper adjustment, from half a second to two seconds, beyond which, beats could be regularly omitted from the electric circuit to be described, thus securing intervals of any length desired. The lower end of the pendulum-rod bore a platinum needle that at each swing made electric connection, at the centre of the pendulum arc, with a mercury meniscus. This pendulum, once set in full swing by the hand, would, for medium-length intervals, preserve regular beats for a far longer time than any single set of experiments, without any discoverable variation whatever. Great care was taken at each change of the interval to adjust the 'bobs' and mercury contact so as both to make the interval of exactly the stated length, and the back and forth swings precisely equal, these being the two matters needing the nicest adjustment in all pendulum motion. The pendulum was introduced into the same electric circuit with an ordinary telegraph key, a telegraph sounder, and a Deprez signal which wrote on the drum of a Ludwig kymograph with automatic spiral thread for the revolving drum. Another Deprez signal wrote the vibrations of a tuning fork upon the same drum, by means of a separate circuit and a König contact. For adjusting the intervals and beats for the first time, a fork of 100 double vibrations was used; the adjustment was extended through one hour, until a beat was secured, the sum of whose error was indistinguishable for that space of time, and therefore the error for any set of experiments practically zero. Two other pendulums were also made for shorter intervals, one of them giving quarter seconds. Any two of these pendulums could be introduced into different loops of the same circuit, and each being adjusted to a different interval, either of the intervals could by means of a bridge, be sent through the same sounder at the will of the operator and without stopping either pendulum; or again at will both pendulums could be cut out of the circuit altogether. The reproductions or judgments of the person undergoing experimentation were expressed by a slight movement of the finger upon an electric key that, by another Deprez signal in a separate circuit, recorded the judgment upon the kymograph drum. Thus during each set of experiments three electric signals with points arranged

over one another, precisely in the same line at right-angles to the motion of the drum, continuously wrote their separate records as follows: Number one recorded the vibrations of a tuning-fork; number two, the beats of whichever length of interval the subject was hearing from the pendulum sounder; and number three, the judgments of this interval expressed by the subject. The tracings on the drum were "fixed" and preserved.

As above stated the length of the reproduction was measured by tuning-fork vibrations written upon the drum; for all the experiments except those of table E, a fork was used making 50 double vibrations per second, thus recording hundredths of a second; for table E, which concerns intervals longer than the others (1.75), a fork of 25 double vibrations, recording fiftieths of a second was used. Many methods were tried for saving the enormous labor of counting these vibrations, which task, together with its strain upon the eyes for such a long series of experiments as the present, can only be appreciated by one who has tried it for several months. The slightly irregular motions of the kymograph make it entirely inaccurate merely to scale the intervals. The quickest and safest method of counting we discovered was as follows: When the paper is cut from the drum it presents on the sheet several parallel lines. Several scales were made fitting all the degrees of irregularity which the fork vibrations in these lines from time to time displayed; one of these scales was then selected to fit each line, part of a line, or set of lines according to their variation; usually three, and often one scale would fit the fork-record of a whole sheet; the eye quickly detects, after some experience, whether the scale fits or not, and thus enables the counting of the vibrations by using the scale as a tally, with comparative facility and absolute accuracy.

It is an important feature that in all experiments to be reported, great pains was taken to keep the persons experimented upon, in entire ignorance of the character of their judgments, or of any of the 'points' or the nature of the experiments whatever, in order to secure absolute freedom from unconscious prepossessions or subjective influences; where

this was not accomplished, as was necessarily the case in two instances, (subjects S. and L.), there was from the character of the men a minimum probability of subjective prepossessions. Moreover as by far the greater majority of the subjects were thus precluded from prepossession until their tests were completed, and as the records of the few who were not so precluded, including those upon myself, entirely accord with those who were, we think the results are reasonably free from this too usually neglected source of vitiation.

Method: The first class of experiments was conducted as follows: The subject was always seated alone in a noiseless room; the electric sounder and the recording key, both on a table before him, were the only apparatus within his sight or hearing; the former brought him through one circuit the beats of the metronome in sharp metallic strokes of uniform strength; with the latter he recorded his judgments upon the kymograph drum in another room. In the latter room with the kymograph was also the pendulum and remaining apparatus, presided over by an assistant. The precise method of these A experiments was invariably as follows: (1) The pendulum was started with full swing, giving beats .75 in length, the electric circuit remaining open. (2) "Ready" signals passed between assistant and subject. (3) Kymograph and tuning-fork were started. (4) The assistant closed the pendulum circuit long enough to send to the subject six beats, or five intervals of .75 each. (5) The assistant opened the pendulum circuit, silencing the sounder. (6) The subject meantime had sought to catch the beat of the sounder from the first beat of the norm and simultaneously to reproduce the beat upon his recording key during the 6 beats of the norm. After the sounder ceased, he continued to reproduce the interval, without breaking the continuity of the series, according to his closest judgment, these reproductions being recorded continuously by the proper circuit upon the drum. (7) The assistant permitted the subject to continue his reproductions until the drum had exhausted the full length of its spiral, when he signalled "stop." The drum was set to exhaust its spiral in two minutes; thus through all classes of experiments to be reported, the reproductions were extended through approx-

imately the same space of time, though of course the number of reproductions varied according to the length of the intervals used and the judgments made. Frequently short portions of the spiral would be used in adjustments of the signals or by accidents, so that the time actually used was shortened more or less. (8) After a few moments of rest a new beat, .9 long, or 20% longer than the norm was sent in to the subject, which with closest possible attention and care he strove to reproduce simultaneously, stroke exactly with stroke, during three minutes. No record was made on the drum of this exercise or practice. (9) A fresh drum having been put in the kymograph by the assistant during the above exercise, immediately upon the expiration of the three minutes, a signal was given to the subject to cease practicing. (10) A new series of 6 beats of the original norm of .75 was then given, and the above numbers (1) to (7) inclusive were repeated precisely as in their first order. In other words a new drum-full of reproductions of the .75 was obtained under precisely the same conditions as the first, with the exception that the first series was "Without practice" or exercise upon any particular interval, while the second set was under the immediate influence of 3 min. practice upon an interval 20 per cent. longer, *i. e.* on .9 (11) After a proper rest, still a third series or drum-full was taken precisely as before, except this time after like practice upon an interval 20 per cent. shorter than the norm, that is on .6

Thus was obtained at each "sitting," though with proper rest between each series, three sets of judgments, as follows: (*a*) without practice; (*b*) after 3 min. exercise upon .9 intervals; (*c*) after 3 min. exercise upon .6 intervals. Table A is arranged to show the comparative results of these three sets.

TABLE A.[1]

Norm, .75 sec. Practice, 3 min. each on .9 sec. and .6 sec. (20 per cent. longer and shorter). Trials, 17. Persons, 6.

(0), (+) and (—) indicate average reproductions made after hearing 6 beats, separated by a normal interval of .75 sec. (0) indicates averages made without practice; (+) after 3 min. practice on 9 sec.; (—) after 3 min. practice on .6. Where the (+) figure is greater than the corresponding (0) figure or the (—) less then the corresponding (0),

[1] The exigencies of space in the JOURNAL require the withholding of still more detailed tables carefully prepared and in the author's possession.

the figures are printed heavy, to show that these figures follow the rule that practice on a longer interval lengthens the judgment and practice on a shorter interval shortens the judgment as expressed in a following effort to reproduce the standard interval. The letters heading the vertical columns are the initials of persons acting as subjects. The small figures under each initial give the number of experiments from which the averages are made.

Set.	S. 5	L. 3	C. 3	F. 3	A. 2	N. 1	General Averages 17
(0)	.712	.607	.750	.735	.671	.814	.712
(+)	.710	**.663**	.727	**.757**	**.749**	.801	**.723**
(—)	.715	.614	**.697**	**.706**	.680	**.731**	**.689**

Results: With normal interval of .75, the general average of 17 tests upon 6 persons shows that there is a very slight and uncertain tendency to follow the rule that three minutes previous close attention to, and simultaneous reproduction of, intervals respectively 20% longer or shorter than the norm, correspondingly lengthen or shorten the judgment; that is, that the habit formed by the practice holds over to influence the succeeding judgments but slightly, if at all.

Series A being deemed inconclusive, it was followed by Series B, the only changes made being first, that a norm of 1.25 was used in place of .75, and second, that only two sets of reproductions were taken, namely: one without practice (0) and one after three minutes practice (—) on an interval of .25.

TABLE B.

Norm, 1.25 sec. Practice, 3 min. on .25 sec. Trials, 60. Persons, 12. At the head of each vertical double column is the initial of the subject. In the left hand column are the numbers of the single experiments from which the averages in the other columns are computed. The columns headed (0) contain average judgments of the 1.25 norm made without practice; those headed (—) similar judgments made after three minutes practice on a .25 beat. This table shows the average for each set of each individual, and also the general averages of each individual and of the total experiments of this table. The averages for this table are computed from the full number of reproductions of each drumful.

No. of Trial.	N.		S.		C.		L.		F.		A.	
	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)
1	1.423	1.328	1.332	1.275	1.244	1.146	1.245	1.166	1.597	1.517	1.138	1.212
2	1.453	1.303	1.470	1.403	1.229	1.169	1.238	1.091	1.437	1.425	1.379	1.319
3	1.530	1.317	1.748	1.343	1.278	1.276	1.222	1.258	1.491	1.446	1.368	1 349
4	1.321	1.252	1.313	1.306	1.316	1.307						
5	1.376	1.248	1.493	1.176	1.281	1.253						
6	1.333	1.287	1.519	1.328	1.335	1.280						
7	1.278	1.252	1.625	1.550	1.323	1.267						
8	1.249	1.189	1.515	1.522	1.334	1.307						
9	1.234	1.216	1.282	1.156	1.437	1.312						
10	1.346	1.277	1.226	1.209								
11	1.285	1.229										
12	1.336	1.185										
13	1.245	1.209										
14	1.350	1.196										
15	1.396	1.164										
16	1.337	1.265										
17	1·278	1.198										
18	1.296	1.186										
19	1.362	1.257										
20	1.381	1.287										
Gen'l Aver-age.	1.335	1.242	1.435	1.313	1.306	1 253	1.236	1.167	1.506	1.461	1.291	1.290
Differ-ence.	–.092		–.121		–.053		–.068		–.044		–.001	

TABLE B.—Continued.

No. of Trial.	W.		M.		Sh.		K.		Ca.		D.	
	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)	(0)	(—)
1	1.137	1.201	1.169	1.189	1.352	1.275	1.311	1 280	1.138	1.118	1.441	1.288
2	1.355	1.284	1.239	1.206	1.147	1 216	1.256	1.245	1.146	.954		
3	1.246	1.199										
Gen'l Aver-age.	1.246	1.226	1.203	1.192	1.242	1.241	1.284	1.261	1.142	1.022	1.441	1.288
Differ-ence.	–.020		–.011		–.001		–.023		–.120		–.534	

Results: Total General Average Without Practice, 1.3228"
" " " After " 1.2533"

Difference, .0695"

These B experiments upon the 1.25 interval, show an

almost universal shortening of those judgments which were preceded by three minutes close attention to, and simultaneous reproduction of, beats .25 long, the average difference between the judgments of the two conditions being .0695. The average difference of no individual out of the 12 included in the table varied from the general rule, and only in 6 trials out of the 60 was the rule broken for single trials, and no person broke the rule more than once. In general, those most experienced in laboratory work conformed most strictly to the usual law; the law was most frequently broken upon the first test made upon an individual, this happening 4 times out of the 6; and it may be remarked in relation herewith, that more variations should be looked for from nervousness or other disturbing causes under these conditions, and from those persons with whom they were actually found. In general, also, the amounts of the difference made between (0) and (—) was proportional to the amount of experience the subject had in psychophysical experiment; for instance, those for Dr. Donaldson, Dr. Sanford, Dr. Lombard and myself are among the largest. Curves were drawn for each individual similar to those of the accompanying chart. Study of these discovers that the Constant Error, whether plus or minus, shows itself most frequently to a marked degree, from the very beginning of the reproductions, and nearly always so before the seventh to the ninth beat, or in other words, before the elapse of ten seconds. Also, the Constant Error tends to preserve a uniform course from the beginning, either the judgments growing gradually longer or gradually shorter throughout the drum, according as their value is greater or less than the normal; in those individuals where the Constant Error is greatest and most marked, this gradual increase or decrease is most marked, as with Dr. Donaldson, where is the largest plus value, and with Dr. Lombard, where is next to the greatest minus value of the judgments.

A beat .25 in length was now chosen for the norm, and being shorter and more difficult to catch was always given 10 times as a sample for each set of reproductions, in place of 6 beats, as in the previous experiments. The practice interval was also changed for this table to 1.25, and for a period

of 5 minutes in place of 3 minutes, as formerly. The reason for this increase in the length of the time of practice is manifest when we consider that two factors enter into the functions of practice, namely: first, the number of repetitions which the subject or central cells would be called upon to make during the practice; and secondly the fatigue, nutritive, restorative, or other processes, which may depend somewhat upon the mere length of time which the practice is continued. We know little or nothing of the effects of either factor, but as in the C experiments practice on 1.25″ gave much fewer number of repetitions, the length of practice time was increased from 3 to 5 minutes, which was an indefinite compromise between proportional length of time of practice, and proportional number of beats.

The shortness of the interval would have given a great number of reproductions, since the same length of the drum's spiral was used as before; and the labor of counting so many would have been excessive; therefore, although the subject made his reproductions for approximately the same length of time as in the previous experiments, records were taken upon the kymograph of only the first 40 reproductions, and of a second set of 40, taken after the lapse of one minute from the last beat of the norm. All the other conditions were the same as before, making the method for Table C as follows: (1) Norm of .25 (10 beats given); a drumful of reproductions taken without practice. (2) Practice 5 minutes on 1.25 beats. (3) Norm of .25, (10 beats given); a drumful of reproductions taken after practice.

TABLE C.

Norm, .25 sec. Practice, 5 min. on 1.25 sec. Trials, 30, Persons, 8. Shows averages of each set and trial, of each individual, and the general averages as before. Averages of the first 40 reproductions are marked *a*, of the second 40, *b*; and the average of *a* and *b* is marked *c*; (0) without practice; (+) after 5 min. practice on 1.25.

No. of Trial.		S. (0)	S. (+)	N. (0)	N. (+)	H. (0)	H. (+)	Ma. (0)	Ma. (+)
1	a	.259	.289	.238	.241	.273	.289	.249	.261
	b	.245	.288	.230	.243	.232	.297	.242	.249
	c	.252	.288	.234	.242	.252	.293	.245	.255
2	a	.254	.255	.247	.248	.244	.262	.246	.249
	b	.239	.250	.243	.247	.225	.262	.238	.242
	c	.246	.253	.245	.247	.235	.262	.242	.245
3	a	.249	.251	.256	.248	.242	.259		
	b	.244	.244	.245	.242	.227	.251		
	c	.246	.247	.251	.245	.234	.255		
4	a	.261	.260	.237	.239				
	b	.243	.255	.234	.240				
	c	.252	.257	.235	.240				
5	a	.263	.265	.242	.252				
	b	.258	.255	.234	.248				
	c	.260	.260	.238	.250				
6	a	.259	.263	.246	.254				
	b	.254	.253	.238	.250				
	c	.256	.258	.242	.252				
7	a	.249	.265	.254	.261				
	b	.236	.262	.254	.259				
	c	.242	.264	.254	.260				
8	a	.253	.247	.242	.248				
	b	.256	.248	.217	.234				
	c	.255	.244	.230	.241				
9	a	.249	.246	.237	.246				
	b	.234	.245	.208	.237				
	c	.242	.246	.222	.241				
10	a	.245	.251	.243	.254				
	b	.234	.254	.231	.251				
	c	.239	.252	.227	.252				
Totals.	a	.254	.259	.244	.249	.253	.270	.248	.255
	b	.254	.255	.233	.245	.228	.270	.240	.246
	c	.249	.257	.239	.247	2.40	.270	.244	.250
Differences.	a		.005		.004		.017		.006
	b		.011		.011		.042		.006
	c		+.008		+.008		+.029		+.006

TABLE C.—Continued.

No. of Trials.		Ca.		B.		McD.		T.	
		(0)	(+)	(0)	(+)	(0)	(+)	(0)	+
1	*a*	.244	.252	.255	.253	.248	.249	.258	.252
	b	.233	.253	.246	.250	.251	.252	.256	.245
	c	.238	.253	.250	.252	.250	.250	.257	.249
2	*a*	.246	.251						
	b	.250	.254						
	c	.248	.252						
Totals.	*a*	.245	.251	.255	.253	.248	.249	.258	.252
	b	.241	.253	.246	.250	.251	.252	.256	.245
	c	.243	.252	.250	.252	.250	.250	.257	.249
Differences.	*a*		.006		.001		.000		.006
	b		.012		.004		.000		.011
	c		+.009		+.002		+.000		—.008

Results: Total General Average Without Practice (*a*) .2500; (*b*) .2396; (*c*) .2448.

Total General Average After Practice (*a*) .2557; (*b*) .2525; (*c*) .2542.

Difference (*a*) .0057; (*b*) .0129; (*c*) .0093.

These C Experiments seem to show that 5 minutes' practice upon a 1.25 beat, lengthens judgments of .25 intervals on an average .00935; the result is the more striking and conclusive when the smallness of the average lengthening is compared with its constancy, the "after practice" set of General Averages of the total 30 trials, exceeding the "without practice" set in every instance, and even in averages of three trials, as those of H (a subject who at the time was entirely ignorant of the purpose of his experiments), the "after practice" judgments falling below the corresponding "withouts" but twice out of the 240 recorded judgments. The Curves of the General Averages of the total thirty trials is shown in Fig. III of the Chart, and those of H in Fig. IV. The continuous line in the chart shows the judgments "without practice," and the dotted line "after practice" as previously in Figs. I and II.

TABLE D.

Norm, .75 sec. Practice, 7 min. on 1.75 sec. and 5 min. on .25. Trials, 30. Persons, 8.

This table will be understood without other explanation than that its method was precisely that of Table A, except that the 'long' practice was changed from 3 min. upon .9 to 7 min. upon 1.75, and the 'short' practice from 3 min. upon .6 to 5 min. upon .25; also, 7 beats of the norm were given for the copy from which the reproduction of each set was made. The table shows averages of each set and trial, of each individual, and the General Averages as before. Averages of the first 40 reproductions are marked *a*, of the second 40 *b*, and the average of *a* plus *b* is marked *c*; (0) = without practice; (+) after 7 min. practice on 1.75; (—)= after 5 min. practice on .25.

No. of Sittings.		S.			N.			H.			B.	
	(0)	(+)	(−)	(0)	(+)	(−)	(0)	(+)	(−)	(0)	(+)	(−)
1 a	.725	.786	.710	.801	.803	.747	.798	.764	.666	.705	.685	.648
b	.726	.788	.690	.800	.814	.759	.835	.869	.670	.629	.621	.599
c	.725	.787	.700	.800	.808	.753	.817	.817	.668	.667	.653	.623
2 a	.748	.835	.772	.781	.771	.751	.800	.878	.744			
b	.766	.882	.792	.773	.798	.761	.899	.963	.794			
c	.757	.859	.782	.777	.785	.756	.849	.920	.769			
3 a	.793	.795	.699	.739	.846	.712	.810	.982	.774			
b	.801	.803	.718	.753	.844	.722	.894	1.148	.900			
c	.797	.799	.709	.746	.845	.717	.852	1.065	.837			
4 a	.718	.795	.721	.736	.836	.696	.732	.909	.732			
b	.699	.809	.754	.731	.975	.676	.794	1.056	.779			
c	.708	.802	.737	.734	.906	.686	.763	.985	.755			
5 a	.768	.924	.727	.822	.826	.740	.787	1.003	.736			
b	.746	1.019	.729	.790	.889	.716	.833	1.233	.794			
c	.757	.971	.728	.806	.857	.728	.810	1.118	.765			
6 a	.782	.877	.735	.777	.804	.709						
b	.761	1.120	.705	.775	.810	.704						
c	.772	.999	.720	.776	.807	.707						
7 a	.781	.889	.778	.771	.820	.708						
b	.824	1.118	.819	.770	.990	.693						
c	.803	1.003	.798	.770	.905	.701						
8 a	.714	.816	.716	.761	.972	.722						
b	.704	.822	.696	.798	.997	.727						
c	.709	.819	.706	.780	.984	.725						
9 a	.758	.879	.744	.753	.961	.741						
b	.768	.852	.743	.758	.952	.735						
c	.764	.865	.743	.756	.956	.738						
10 a	.754	.840	.726	.747	.819	.788						
b	.780	.855	.716	.751	.867	.777						
c	.767	.847	.721	.749	.843	.782						
Totals. a	.754	.843	.732	.768	.845	.731	.785	.907	.730	.705	.685	.648
b	.757	.907	.736	.769	.893	.727	.851	1.053	.787	.629	.621	.599
c	.755	.875	.734	.767	.869	.729	.818	.980	.758	.667	.653	.623
Differences. a		+.089	−.021		+.076	−.037		+.121	−.055		−.020	−.057
b		+.149	−.019		+.123	−.042		+.202	−.064		−.008	−.030
c		+.119	−.021		+.100	−.040		+.162	−.059		−.014	−.044

TABLE D.—Continued.

No. of Sittings.		McA. (0)	McA. (+)	McA. (−)	Ma. (0)	Ma. (+)	Ma. (−)	Sh. (0)	Sh. (+)	Sh. (−)	Ca. (0)	Ca. (+)	Ca. (−)
1	*a*	.743	.739	.699	.695	.734	.641	.759	.835	.713	.702	.734	.679
	b	.708	.731	.635	.624	.708	.688	.731	.892	.720	.644	.687	.553
	c	.725	.735	.667	.659	.721	.665	.740	.863	.716	.673	.711	.616
Totals.	*a*	.743	.739	.699	.695	.734	.641	.759	.835	.713	.702	.734	.679
	b	.708	.731	.635	.624	.708	.688	.731	.892	.720	.644	.687	.553
	c	.725	.735	.667	.659	.721	.665	.740	.863	.716	.673	.711	.616
Differences.	*a*		−.004	−.044		+.039	−.054		+.076	−.122		+.032	−.023
	b		+.023	−.073		+.084	+.064		+.161	−.011		+.043	−.091
	c		.010	.058		+.062	+.006		+.123	.024		+.038	.057

General Average of All.			(0)	(+)	(−)
	Totals.	*a*	.758	.838	.722
		b	.762	.897	.725
		c	.760	.867	.724
	Differences.	*a*		+.080	−.035
		b		+.134	−.036
		c		+.107	−.036

Results: Total General Averages Without Practice (*a*) .7583; (*b*) .7621; (*c*) .7602.

Total General Averages after practice on longer beat (*a*) .8385; (*b*) .8971; (*c*) .8678.

Total General Averages after practice on shorter beat (*a*) .7225; (*b*) .7255; (*c*) .7240.

Total General Average Difference after practice on longer beat (*a*) .0802; (*b*) .1349; (*c*) .1075.

Total General Average Difference after practice on shorter beat (*a*) .0358; (*b*) .0366; (*c*) .0362.

Comparison of experiments *A* and *D* shows that, for the same interval of .75, while in the former with a difference of 20 per cent. between the norm and practice intervals the effect of habit or practice was so slight as to be uncertain if active at all, in the latter experiments, with a much greater difference between the norm and practice intervals, the effect

was strong and constant. Figure V of the chart shows the curve of the General Averages for the 30 trials and 8 persons; figure VI shows the curve for Sh., and illustrates a single trial.

TABLE E.

Norm, .5 sec. Practice, 5 min. on 1.75 sec. Trials, 6. Persons, 2.

The only other variation than those in the above line was for these experiments, that 10 beats of the norm were given for the sample from which each set of reproductions was made. Averages of the first 40 reproductions are marked *a*, of the second 40, *b*; the averages of *a* plus *b* are marked *c*; (0) = Without Practice; (+) = after 5 min. practice on 1.75.

No. of Trials.		S.		N.	
		(0)	(+)	(0)	(+)
1	*a*	.517	.541	.491	.521
	b	.503	.518	.484	.515
	c	.510	.530	.487	.519
2	*a*	.491	.528	.503	.523
	b	.505	.559	.483	.516
	c	.498	.544	.493	.520
3	*a*	.497	.535	.489	.498
	b	.504	.551	.484	.477
	c	.500	.543	.487	.487
Totals.	*a*	.501	.534	.494	.514
	b	.503	.542	.466	.502
	c	.502	.538	.480	.508
Differences.		+.033		+.020	
		+.039		+.035	
		+.036		+.028	

Results: Total General Averages Without Practice (*a*) .4980; (*b*) .4852; (*c*) .4916.

Total General Averages after practice on longer beat (*a*) .5246; (*b*) .5228; (*c*) .5237.

Total General Average difference (*a*) .0266; (*b*) .0376; (*c*) .0321.

Figure VII of the chart shows the curve for the general averages of the six tests of these experiments on the interval .5.

TABLE F.

Norm, 1.75. Practice 6 min. on .5. Trials, 6. Persons, 2.
Seven beats of norm given for sample to be reproduced. (0) = Without Practice; (—) = after 6 min. practice on, .5 sec.

No. of Trials.	S.		N.	
	(0)	(—)	(0)	(—)
1	2.03	2.14	2.19	1.83
2	2.06	2.36	2.24	1.79
3	2.31	2.35	1.88	1.75
Totals.	2·136	2.284	2.049	1.790
Difference.	+.048		—.259	

Results: Total General Average Without Practice, 2.089
" " " After " 2.010

" " " Difference, — .079

It will be observed that the three trials of S. for this interval are all contrary to the usual law; whether this is accidental and due to the small number of the trials, or if practice is less efficient in its influence upon judgments of long intervals, is undetermined; we incline to believe the former.

At this point in the experiments it appeared conclusive that a certain amount of sustained exercise, with close attention to the repetition of definite beats heard from a pendulum or sounder, and reproduced by motion of the finger upon a key, induces some sort of more or less permanent effect or habit, whose influence unconsciously modifies accordingly the judgments or reproductions of other beats heard and reproduced immediately after such exercise or practice. The question now arose whether this effect was muscular or "central." To determine this, the following experiments were instituted; their method was the same as the foregoing except that in place of hearing the beats of the sounder the

armature or stroke-bar of the latter was pressed lightly between the thumb and forefinger of the *left* hand; the soft parts of the balls of the fingers were intruded slightly between the bar and the anvil or brasses between which the bar played, and, the circuit being closed, each time the pendulum made a stroke a "pulse-like" sensation was felt by the fingers. The left hand, thus holding the sounder, was then rolled in several thicknesses of cloth and folded with a woolen coat, and the ears closed with cotton or wax till no noise from the sounder could be heard with the closest possible attention. Also, the practice was now exercised or received in a purely afferent manner, without repeating the practice interval upon the key, simultaneously with the beats of the sounder as was done in the other experiments. By these means the effect of the practice was confined afferently to the left thumb and forefinger, and to their respective nervous centres. The reproductions of the trial intervals, both the set previous to practice and the correlative set after practice, were made with the right hand or fingers, as in all previous experiments.

It is evident that if similar effects from practice should manifest themselves under these conditions as in the former experiments, the cause could in no way be attributed to a muscular habit, because no muscles were at all concerned in the reproductions of the normal or trial intervals, which had been in any way influenced by the previous afferent exercise on the practice interval. Of course it is possible that every afferent impulse occasions some efferent discharge, although the same be actively ineffectual; yet even if this did happen, we think it would be fair to assume that the cause of the difference between the two sets of judgments was central and not muscular.

TABLE G.

Norm, 1.25. Practice, 6 min. on .25. Trials, 50. Persons, 16.

Practice taken by touch alone in left thumb and finger, the beat being inaudible. Ten beats of norm given as sample for all reproductions. (A) = Averages without practice; (B) = Averages after 6 min. practice on .25 beats; D = difference between (A) and (B).

TABLE G.

No. of Sittings.		S.	N.	L.	H.	C.	A.	D.	B.
1	*A*	1.276	1.200	1.065	1.480	1.339	1.146	1.433	1.442
	B	1 215	1.109	.942	1.530	1.390	1.111	1.437	1.334
	D	− .061	− .091	− .123	+ .050	+ .051	− .035	+ .004	− .108
2	*A*	1.302	1.174	1.025	1.443	1.564	1.396		
	B	1.202	1.033	1.238	1.410	1.475	1.393		
	D	− .100	− .141	.203	.033	− .089	− .003		
3	*A*	1.247	1.140	1.358	1.447	1.289	1.446		
	B	1.195	1.029	1.159	1.282	1.438	1.480		
	D	− .052	− .111	− .199	− .165	+ .149	+ .034		
4	*A*	1.207	1.082	.883	1.388	1.307	1.493		
	B	1.350	1.058	.882	1.223	1.255	1.420		
	D	+ .043	− .024	− .001	− .165	− .052	− .073		
5	*A*	1.390	1.245	1.093	1.333	1.277	1.553		
	B	1.281	1.071	1.012	1.233	1.284	1 369		
	D	− .109	− .174	− .081	− .100	+ .007	− .184		
6	*A*	1.174	1.179						
	B	1.109	1.085						
	D	− .065	.094						
7	*A*	1.350	1.326						
	B	1.353	1.202						
	D	+ .003	− .124						
8	*A*	1.322	1.261						
	B	1.289	1.089						
	D	− .033	− .172						
9	*A*	1.583	1.271						
	B	1.335	1.154						
	D	− .248	− .117						
10	*A*	1.334	1.213						
	B	1.361	1.031						
	D	+ .027	− .182						
Totals.	*A*	1.310	1.210	1.061	1.416	1.347	1.389	1.433	1.442
	B	1.262	1.083	1.029	1.326	1.363	1 312	1.437	1.334
	D	− .048	− .126	− .032	− .090	+ .016	.077	+ .004	− .108

TABLE G.—Continued.

No. of Sittings.		McM.	W.	Ha.	M.	Mi.	Ma.	Hn.	Ca.	Average of last 10.
2	A	1.301	1.350	1.408	1.372	1.425	1.029	1.183	1.268	1.306
	B	1.163	1.210	1.397	1.474	1.322	.906	1.194	1.161	1.245
	D	— .138	— .140	— .011	+ .102	— .103	— .123	+ .011	— .107	— .061
Totals.	A	1.301	1.350	1.408	1.372	1.425	1.029	1.183	1.268	1.306
	B	1.163	1.210	1.397	1.474	1.322	.906	1.194	1.161	1.245
	D	.138	— .140	— .011	+ .102	— .103	— .123	.011	— .107	— .061

General Average, $\left\{\begin{array}{r} 1.277 \\ 1.213 \\ \hline -\ .064 \end{array}\right.$

Results: Total general average without practice, 1.2776
Total general average after 6 min. practice on, .25, received only through left thumb and finger, } 1.2137

Total general average difference, —.0639

The results of these *G* experiments are particularly to be compared with those of Table *B*, both having had the same norm and same practice intervals. The length of time which practice was undergone, however, was in *G* double that in *B*, which probably should be counted as a reason why the difference between the "without practice" and the "with practice" results should have been greater in *G* than in *B*. An offset to this influence, however, lies probably in the fact that in the *B* practice the intervals were not only afferently received, but also efferently executed, bringing into play the whole psychophysical arc of sensory centers, motor centers, and muscles of the fore-arm, hand and fingers; under these circumstances this arc soon takes on, as a whole, a simultaneous function of a strongly reflex nature, the reproductions not following the beats of the norm, but precisely and spontaneously coinciding with them, beat on beat; the whole process of reproducing here is itself of the nature of an induced habit, and it is natural to suspect that the continuation of the habit, sustained through the term of practice, would have a stronger and more lasting

effect than where the sensory centers alone were exercised, as in the *G* experiments. What the precise results of these countervailing conditions may have been we cannot determine, but the very close equivalence of the total differences of the two tables (— .0695 for *B*, — .0639 for *G*), is very likely to have been within certain limits accidental.

It is not likely that the same experiments repeated under conditions as nearly as possible like these, and upon the same individuals, would produce precisely the same results, for the human organism, mental and physical, is so complex, its environment so variable, the entire conditions of the problem so multifariously changeable, that the mathematical probabilities are almost infinitely against identical combinations. But results constantly like in nature and approximately like in degree, should, we think, be deemed scientifically acceptable. Even with these, the time-problem is so difficult and so liable to subjective and delusive complications, that we cannot look upon the experiments here reported, (as extensive, careful and conclusive as we have endeavored to make them,) as being entirely conclusive until they shall have been confirmed by similar work of other experimenters. With these provisions, however, we think the results of the foregoing experiments indicate that, *sustained attention to a rythmically repeated impulse induces in the corresponding nervous centre a habit or tendency to continue that impulse, which habit influences, or modifies succeeding time-judgments.*

The following table summarizes our results with reference to the Constant Error. We have thought best to give the length of the judgments rather than the amount of the error; the plus sign is prefixed to those judgments which are greater, and the minus signs to those which are less than their corresponding norm; also, the table shows the number of trials from which each average is calculated, and the table from which the same are taken. The judgments of Table *H* are alone the first series of each set or trial, that is, those made without practice or normally.

TABLE H.—CONSTANT ERROR.

Persons.	Norm .25		Norm .50		Norm .75		Norm 1.25		Norm 1.75	
	No. Trials and Table.	Average Judgment.	No. Trials and Table.	Average Judgment.	No. Trials and Table.	Average Judgment.	No. Trials and Table.	Average Judgment.	No. Trials and Table.	Average Judgment.
					5 A	— .712	10 B	+1.435		
S.	10 C	— .249	3 E	+ .502	10 D	+ .755	10 G	+1.310	3 F	+2.136
					Average	— .741	Average	+1.373		
					1 A	+ .814	20 B	+1.335		
N.	10 C	— .239	3 E	— .480	10 D	+ .769	10 G	—1.210	3 F	+2.049
					Average	+ .773	Average	+1.293		
							3 B	—1.236		
L.					3 A	— .607	5 G	—1.061		
							Average	—1.126		
							9 B	+1.306		
C.					3 A	+ .750	5 G	+1.347		
							Average	+1.321		
H.	3 C	— .240			5 D	+ .818	5 G	+1.416		
							3 B	+1.291		
A.					2 A	— .671	5 G	+1.389		
							Average	+1.353		
							2 B	—1.142		
Ca.	2 C	— .243			1 D	— .673	1 G	+1.268		
							Average	—1.184		
Ma.	2 C	— .244			1 D	— .659	1 G	—1.029		
Sh.					1 D	— .740	2 B	—1.242		
							2 B	—1.203		
Mi.							1 G	+1.425		
							Average	+1.277		
F.					3 A	— .735	3 B	+1.506		
W.							3 B	—1.246		
							1 B	+1.441		
D.							1 G	+1.433		
							Average	+1.437		
B.	1 C	+ .250					1 G	+1.442		
K.							2 B	+1.284		
McD.	1 C	+ .250								
T.	1 C	+ .257								
Bl.					1 D	— .667				
McM.							1 G	+1.301		
W.							1 G	+1.350		
Ha.							1 G	+1.408		
M.							1 G	+1.372		
McA.					1 D	— .725				
Hn.							1 G	—1.183		
					17 A	— .712	60 B	+1.322		
General Averages.	30 C	— .244	6 E	— .491	30 D	+ .760	50 G	+1.277	6 F	+2.089
					Average	— .742	Average	+1.302		

Results: With the method used, the experiments, on the whole, indicate that the judgments of intervals of .75, .50 and .25 are very slightly shortened, while those of 1.25 and 1.75 are considerably lengthened. Too few intervals were used to determine the Indifference Point accurately, yet in view of the great variations displayed, we may perhaps come as near the truth as can be well attained, if we calculate this point, for these experiments, from the General Average of the intervals used; according to such computation the Indifference Point would appear to be about .81. Yet so great are the individual differences and even the variations of the Constant Error from time to time for the same individual, that this error should be termed Inconstant rather than Constant, and as calculated from any number of persons yet experimented upon, must be considered as extremely problematical and uncertain. Particularly so, as we entirely lack any sure clue to its probable cause. In view of the indication arrived at, that the phenomenon is central, we might infer that the lengthening of the judgment was due to an inertia or tardiness of the centres to repeat the proper rhythm, and that this might be based upon a failure of response in nutritive processes; but this would be difficult to reconcile with the fact that the more rapid intervals, which would be supposed to exhaust the centres most quickly, display the opposite tendency and act more quickly than they ought. Or perhaps the relations between the nutritive and active functions of the centres, are an automatically compensating mechanism, wherein the supply is sometimes "over corrected" and again "under corrected" with reference to the exhaust, just as the balance wheel of a watch is often at fault with reference to temperature, and the watch varies with the season and with the pocket it is carried in; so the time-mechanism of the nervous centre may vary with individual and physical conditions, and with the coat we wear; surely the psychical timepiece is not less delicate or complex than its horological rival of human skill.

Comparing our own results with those of former experimenters, though we learn next to nothing of the cause of the Constant Error and too little of its course to predict

dict the same with any great probability, for any certain person or number of persons; yet study of our tables, and still better of the original curves and charts too numerous to publish, reveals a few points of considerable certainty. Those individuals who make the largest constant error, make the error most constantly in one direction; such persons, also, are apt to make a constantly increasing error throughout the series of reproductions of each drumful; this phenomenon betrays itself even more conspicuously in the "after practice" series than in the "without practice" series; the phenomenon is illustrated in the judgments of L and of H in Table *G*, and in their respective curves, Figures X and XI of the chart; judgments of the former are unusually short throughout the experiments, and in the curves, show themselves growing rapidly shorter and shorter to the end of the drum; the judgments of H are unusually long throughout all his trials, and his curves go rapidly up throughout each drum. This raises a serious question as to what the magnitude of the Constant Error would be for a longer and different period of reproduction. Possibly, also, this point has relation to the fact that contrary signs are found for the Constant Error by the German experimenters who used single reproductions, and by Mr. Stevens (with whom my results pretty closely agree) and myself, who used multiple reproductions. Examination of the first reproduction of each drumful of my work, does not discover the contrariness of sign for Constant Error, between the first and the subsequent judgments of the series, which would correspond to the contrariness of results between the above mentioned experiments with single and with multiple reproductions. New experiments seem needed for the tripartite relations between the sign of Constant Error, the number or length of time the norm is given as a sample, and the number of the reproduced judgments.

Another feature of interest is, that any slight nervousness or excitement of the subject shortens the judgments. Often the subject who sits for the first time, looks upon any psychological experiment as in some way a test of mental caliber; this, together with fresh interest in the experiment, occasions a slight eagerness, excitement, or mental tension for the first

trial, which is not so much, if at all, present in future ones. Examination of results taken under such conditions, convinced me while the experiments were in progress that they were shorter than the ordinary ones. It is evident that this, if true, would have bearing upon the method of our experiments; for instance, if in first sittings the average judgments of the first or "without practice" trial be for the above reason shortened more than the following "after practice" set, allowance ought to be made for this in estimating the shortening or lengthening effect of the practice upon the later set; otherwise, in those cases where the practice interval was shorter than the norm, the shortening effect of the practice in the "after practice" set would be negatived to the extent of the shortening due to excitement in the "without practice" set, and the reverse for practice intervals longer than the norm. Examination of the tables shows that the law, that the "after practice" sets are longer or shorter than the "without practice" sets, according as the practice interval is longer or shorter than the norm, is broken to a more or less degree in 48 out of 246 times; 17 out of these 48 digressions occurred in first sittings, and 11 out of these 17 occurred in those experiments where the practice interval was shorter than the norm. This is in accordance with what has been said regarding excitement, yet a more detailed scrutiny of the results than is possible to give here, is chiefly the ground for what we have stated on this point.

Much has been said by previous experimenters concerning the effects of attention. Undoubtedly with single reproductions sensibility and accuracy are directly proportional to the attention given; with multiple reproductions it is doubtful if this is the case for the expert and experienced subject. For myself, who have had very unusual experience, my best judgments are made by paying the greatest possible attention to the norm during the sample beats, and then, when the rhythm is once caught, abandoning myself to as near an unconscious or reflex condition as possible, letting the idea or habit of the rhythm run its own course undisturbed, as near as may be, by attention, volition, or any kind of thinking whatever.

Subjective opinions of one's own judgments: After finish-

ing each drumful the subject throughout the experiments was usually asked his opinion of how well he had kept his copy or norm; only in a small and uncertain number of cases were these opinions found to agree with the truth, and frequently were directly contrary.

How long before the effect of practice shows itself as against the immediate memory of the norm? The results are so variable that this question cannot be answered with precision; nearly always the effect of the practice is exhibited in the very first reproduction to a marked degree; before the expiration of 8 or 10 seconds the effect would seem to be in full force or tendency, from which time forth, the judgments where the Constant Error was well marked, gradually grew longer or shorter to the end of the drum, as we have before stated.

How long does the effect of practice last? Our method did not permit us to observe a longer period than from 1.5 to 2 minutes; the practice seemed to preserve its effect with nearly, if not entirely, its full force for that length of time.

Fatigue: A few experiments were made preserving closest possible attention to the beats and judgments for several hours at a sitting; sample tests of the judgments were taken from time to time. So far as these go, fatigue could not be discovered to have any effect whatever.

Long Experience in making time judgments has been thought by Mehner and others to lessen the Constant Error. Study of the above experiments according to their dates on the protocol, which also agree with the order of the tables as published, discovers very uncertain evidence for this opinion, a slight probability perhaps inclining in its favor.

Mr. Stevens noticed in his work, that judgments of unusual length or shortness are apt to be corrected in the following judgment, "according to a standard which the mind carries, but to which the hand (or perhaps the will during the interval) cannot be accurately true." To a certain degree the same phenomenon is observed in my charts and curves, though I am rather inclined to carry back the cause to some automatically compensating adjustment of the rhythmic habit or

function of the nerve centres, than to the vague phrase "a standard carried by the mind."

Anomalies: Seeing no just reason for the culling out of anomalies in former experiments, I have permitted none in my own. Every test taken in the course of any regular experiment has been reported in its proper place, with the exception of a single trial each, for three persons, who, from nervousness (one was a young woman) or lack of rythmic sense, were entirely unable to catch the beat of the norm in a way that would enable them to repeat it with any sort of regularity or likeness to the original.

Sensibility: Owing to the enormous labor that would be involved in computing the Average Error for so many judgments, no investigation was made by me of this factor. On the whole, however, I should say the nearly uniform results regarding sensibility of all former experimenters, which constitute almost their sole point of agreement, are entirely confirmed by the experiments here reported.

In closing this account of my experiments I have pleasure in thanking those who have given me so much valuable time, taken from their own University work, in acting as subjects for such a tedious and time-robbing investigation, and those also who have assisted me by suggestion, counsel and inspiration.

IV.—CONCLUSIONS.

Sensations and their images or reproductions have various attributes; qualitatively they are blue, or warm, or painful etc.; intensively they are strong or weak, bright or faint, etc. Duration, or continuation, is another attribute or characteristic of every sensation and of every image. This attribute is the ultimate and essential datum of time. Besides sensations and images, science infers and assumes the real and separate existence of certain physical elements, having fixed correlations with each other, and with sensations and images. Whether the grounds for this assumption are acceptable or not we need not here discuss; but according to this assumption, duration or continuing is also an attribute or characteristic of these physical elements, and therein forms a further

field of this ultimate and essential time datum. Again most philosophies, and, I think, all religions and all science, assert, infer, or assume the existence of some soul or super-psychical cause, as an ultimate element separate from, or as a further attribute additional to, the physical elements and the sensations and images; according to these grounds there is thus another field of this characteristic time datum. Thus our time datum is seen to be an attribute belonging to, and inherent in, everything that is conceived to exist. As such, also it is seen to be an ultimate datum; as much so as the blueness, the chilliness, or the painfulness of any sensation, or the existence of anything at all. Why things exist at all, or why their inherent nature is what it is, we think to be at present beyond human explanation. The fundamental datum of our present explanations, then, we shall state to be that time *is* this attribute of duration wherever it exists.

This being the nature of time, what constitutes a perception of time? Hoping the results will justify the use, we shall accept that nomenclature according to which it is said that every elementary sensation or image is *perceived* which presents itself in consciousness at all. When a sensation or image properly occupies the focus of attention, we shall say it is *apperceived.* According to this terminology, time is perceived whenever any sensation or image durates[1] in consciousness at all; it is apperceived when the duration properly occupies the focus of attention. Thus if we suppose a creature so simple as to be without memory, and capable from time to time of but a single elementary sensation of constant quality, say a pain, (such perhaps are some infusoria) we should say that pain was *perceived* whenever it occurred; we should not say it was *apperceived.* We should also say such a creature *perceived* time.

Why sensations ultimately differ at all, why some are red and some blue, some bright and some faint, or why some are long in duration and some short, is beyond explanation. That some are long and some short is an ultimate datum, and no more wonderful than that sensations are diverse in any other

[1] I have coined this word, finding no other sufficiently simple in meaning.

way. But in the same way as we shall say of our simple creature, that he perceives his sensation when it exists at all, and that he perceives time when it (the sensation) durates at all, so we shall say he perceives a certain definite time when it durates in that certain definite manner. Its perception is its occurrence; the ultimate nature of its occurrence, constitutes the ultimate nature of the perception; the definiteness of its occurrence, of its inherent nature, constitutes the definiteness of that certain perception. We know nothing of the perception of such a creature except by inference and analogy; but in the same way that we should say his sensation is painful, in that same way we should say one of his perceptions was five seconds long. And in the same way that we have said he perceives time when his sensation durates at all, so we shall say he perceives five seconds when it durates five seconds, and perceives one second when it durates one second. But according to this, one thing above all else must be carefully noted, *perception* or perception of time duration is *always a process and never a state; a certain definite time is a certain definite process.* We can no more discover an explanation of our perception of the duration of five seconds alone in some mysterious momentary mental arrangement or "temporal sign," or other *instantaneous* characteristic, than we can discover redness in blueness; for us to perceive blue, there must be blue; for us to perceive duration, something must durate; for us to perceive a definite blue, there must be a definite blue; for us to perceive five seconds, something must durate five seconds; for us really to *perceive* a year, some definite sensation would have to durate a year. What takes place when we say we have an *idea* of a year is another matter which we shall discuss in its place.

So also of series of sensations. That series occur at all is an ultimate fact or datum. What actually occurs when a series occurs we shall call a perception of a series. And in the same way as we can never perceive a half-second except something durate a half-second, so we can never perceive a series of five half seconds with intervals of half seconds between the terms, unless such a series occurs. When it occurs its entire occurrence will constitute its perception. Actually

to perceive such a series a year long, such a series would actually have to occur throughout a full year. What takes place when we have an idea of such a series we shall also discuss in its turn.

Neglecting for the present any consideration of the correlation between them, or of any perception of such a correlation, all that we have said regarding sensations applies as well to images or reproduced sensations; really to imagine five seconds, some image must last five seconds; fully to imagine a thousand clock-ticks, a thousand clock-tick images must pass through the mind. So also, fully to remember a thousand second-beats, a thousand second-beat images must pass in full mental review.

And as of pains, and clock-ticks, and second-beats, so of all other mental content whatsoever and however disparate. Mental process is mental perception; every definite or certain process or procession is a definite and certain perception; and every definite perception is also a definite time perception. Yet we must not forget that according to the nomenclature we are now using, perception is not apperception, and a definite time perception is by no means an apperception of a definite time; this we shall come to later.

What we have said of perception applies as well to memory. But when we *say* we remember an occurrence, we seldom, and indeed never, except the occurrence is short, simple and of recent happening, remember it as accurately and fully as it actually transpired. That is, in its re-presentation in memory, some of the items drop out of the process, or rather fail to drop into it; and the remainder stand *unsuspected* for the former whole — do so for the very reason that the former whole now is not, nor can be suspected at all, except in and through so much as *is* re-presented. I may have spent the whole of yesterday listening to the second beats of a clock, yet I may quickly remember that I did so, without the entire day and each tick repeating itself in full or in any instantaneous miniature of fullness in that quick remembrance. But in this quick remembrance, it is probable the entire mental procession of the previous day was re-presented alone by some momentary flash-picture, as it were, of myself as I was seated

at some particularly striking moment of yesterday, listening to the clock; perhaps this flash-picture or remembrance lasted long enough to take in no more than two represented ticks of the clock; perhaps to take in but one; or it may be that *all* the image-ticks were left out entirely and only the word "tick" or "clock" occupied their place in the quick remembrance; for such, it seems quite certain, is the nature of much of our thinking. If that in the above quick remembrance which occupies the place of, stands for, indicates, or symbolizes the original series be named the *idea* of that series, then the idea of that series is not a full representation of that series in any way. And it is plain also that we have in such an idea no such occurrence as that described by Herbart, or Mr. Ward, or any of those who conceive that an idea of a series, or of succession, or of time, must be some sort of instantaneously painted picture presenting the whole length of the time or of the series in a simultaneous perspective. Indeed if needed at all, there would seem to be needed as much such an instantaneous sidewise view of the duration of the simplest sensation and of the briefest part of time in order to perceive that it durate at all, as to perceive that it durates for, say, five seconds. The classic question therefore whether the idea of succession is or is not a succession of ideas, in so far as the question is one as to whether the idea is a longitudinally passing *process*, or a sidewise presented *state*, may as well be fought out with reference to the nature of any original sensation and for the briefest temporal portion of it, as with reference to any train or series of such sensations. Whether a sensation, an image, or a series of such, it does not matter; the pertinent question is, do we perceive the length of any duration, however long, by the *process* of that duration itself, or by some non-processional representative state? The chief arguments or suggestions I have been able to formulate or to find formulated for the "state" theory, all root, it seems to me, in the delusive catch-phrase, "We can not *now* perceive something that is *really past*, therefore our perception of past must be a *present* perception, *i. e.* a state." But this phrase is a series of verbal mis-statements and bad logic from beginning to end; we do not "*now*" perceive this some-

thing, whatever it is, but so far as I can discover we "now-now-now-now" perceive it; we do not stand still and look along the line to measure this past in a perspective view, but *run* along the line as it were (a new line representing the old) to measure it inch by inch, or present by present, by a moving process over again; nor is this something that we re-measure a "really past," nor in the absolute sense do we *re*-measure at all; but the "really past" and the original measuring both gone forever, a new representation of the gone past and a new measuring of the new representation happen "brand-new;" happen in original *representation* of them, though not in re-*presentation* of them. All this being so, our phrase carried out in good logic should read "We *can not* 'now' perceive something that is really past, therefore our 'perception of past' must *not* be a present perception, *i. e. must* be a *process*. On the other hand, the evidence for the opposite or "process" explanation seems to me consistent and even positive. I think that every one who will observe his own mental process when he seeks to measure or to realize the length of any durating sensation or its representation in memory, will easily observe that he never fully perceives or remembers the length instantly or even approximately so; unless, of course, the duration is itself instantaneous or approximately so. On the other hand I think any one will easily convince himself that *fully* to perceive or to remember the length of its representations, these representations must stretch themselves out through an equal process and lapse of time as did their original occurrence. 'Quick ideas' of the nature described above may delusively flash upon us with approximate instantaneousness, but never a full and complete idea, and the time occupied by the idea will be proportional to its completeness.

Another evidence in favor of the process and against the "state" explanation lies, we think, in the following facts. The items of a long series, say the detailed events of a past hour, never are fully represented to us. It is easy to account for this according to the process theory; many of these details fail to reappear, and as the serial reappearance of those which do reappear is our sole suspicion of their presence, or of their order of appearance past or present, so of those which

fail to appear, we *at the time* have no suspicion of their absence or of the fact that they ever existed. At some other time we may remember further details, and also remember this abbreviated memory, and so become aware that we have dropped items from the latter. But according to the "state" theory, it is difficult to conceive why those causes which give the proper perspective to any part of a series where no items are gone, should not give the proper *perspective* to those items which do appear in a series when some items do not appear, and why such a perspective state would not have much such an aspect as the perspective of a picket-fence where some of the pickets are on and some off. Nor must we imagine such a conscious running-over of yesterday's incidents, as one in which we skip or jump from one incident to the other and álmost feel the shocks occasioned by gaping items, to be just such a broken-fence perspective as we above describe. Surely such a series of shocks are a process and not a simultaneous state, even if we are conscious of the gaps; but how we come to *be* conscious of the gaps in this running perspective, is a complex question entirely separate from the one under present consideration, and one we shall hope to throw light upon later. That we do not have simultaneous picket-fence perspective with pickets visibly off, that is with perspective gaps belonging to lost items, is quite evident in our attempts to recall the precise number of ticks in a given series just heard; where as those who have had much experience must observe, they frequently with confidence think they recall the whole series perfectly and with no consciousness of gaps, though there are gaps.

We are inclined to conclude therefore that by the same process that we perceive the duration of any smallest part of any single sensation, by precisely the same nature of process we perceive the duration of any sensation however long and of any series however long; that the duration of the sensation or series, the perception of the duration, and the perception of the length of the duration are one and identical; that the duration is an ultimate datum, and no more capable or needful of other explanation or of further analysis than the blueness of a blue spot.

But perception of the length of a sensation, the apperception of its length, and the perception and apperception of its length as measured by some other sensation, are different matters altogether, as also are so-called perceptions of past, present, and future, and of other definite time relations, and of dates; all of which we must now consider.

More often than otherwise those definite sensations which come through the focus of the eye are those which determine the immediately following ideas; with great frequency these sensations definitely persist long enough to associate for some time with the ideas which they call up. With less frequency the definite sensations of hearing, touch, smell, and so on down the scale, determine the immediately following associations. Frequently very obscure sensations such as a red spot at the very edge of the field of vision, or the temperature of our teeth direct the association. Or perhaps as often as otherwise the particular mental group which determines the association is not a sensation or procession of sensations, but a definite group of images or procession of images which we may call an idea. Whatever group it is that determines the succeeding association, that group we say occupies the focus of attention, the terminology being evidently derived from the fact that the focus of vision is so frequently also the focus of apperception. Apperception is complete association; the object of association is always the object of apperception, and the object of attention. The focus and the object are always identical. When we apperceive anything we couple it with its most usual associations, that is, memories of its own attributes, qualities, and characteristics. This kind of association *is* apperception. Time is apperceived when any process of duration occupies the focus of attention, is the object of association, and calls up durative associations; that is, memories whose characteristics are particularly of the duration quality or nature.

We must with GREATEST *care distinguish between perceiving time and apperceiving time relation.*

Perceptions of relation are commonly supposed to be involved in the very core of the indissoluble mystery of the unity of the mind. We are deeply aware of the importance

of the subject, yet we have been driven to suspect that the secret of perceived relations is to be found in that they are associative processes of the apperceptive degree or nature and not simultaneous states. This subject is not our main question and we shall discuss it but in so far as is necessary for our explanation of time relations. If two tones precisely alike in quality, intensity, and length, begin precisely together and end together, no relation will be perceived between them. If one begins perceptibly before the other, relations will appear. Without some qualitative or some intensive *change* there can be no temporal *relation*. The occurrence of the change in the qualitative or intensive nature of the perception *is* the perception of the relation; and in the same way as it is not some necessary sidewise simultaneous perspective that constitutes perception of homogeneous duration, but the ever flowing attribute of duration itself, therefore we suspect, that every perception of temporal relation is fundamentally the actual procession of one term of definite quality or intensity followed without gap by another term of different quality or intensity; that actually to perceive any definite time relation or change, such must actually transpire; andfully to imagine or to remember such, the corresponding representation of it must again pass through the mind in full review. Without qualitative or intensive change no series could occur; such change is the essential characteristic of a series; the change *makes* the series. Fully to perceive the relations of the terms of a series the full series must be experienced either in original occurrence or in representation. To perceive that A B C D occurs in the relations a b c d it must occur in these relations. To perceive that B is after A, A must happen, then B. To perceive that A is before D, A must happen before D. To apperceive these relations is something quite different. To perceive that D is present and that A B C are gone, A B C must come and go and D must come; the apperceiving of the presence of D, or of the goneness of A B C, or of the relation of the presence of D to the goneness of A are other matters that need much elucidation.

To apperceive D it must occur, stand in the focus of the mind and call up images representing its qualities and usual

associations; to apperceive it as *present*, it must call up the idea 'present'; the apperceived relation of D to the Present is the occurrence of D followed by some idea of "the Present." For us to understand this relation we must understand "the idea of the Present." The word "present" is one that we associate with the continued presence of any mental content, or more strictly speaking with the durative procession of that content through the mind; thus we can associate the word with a passing image as well as with a passing sensation; but most commonly the word Present associates itself with the bodily group of *sensations* which we call self and with the environmental sensations which happen to be present at the moment that we are so apperceiving 'the Present.' Thus when we apperceive D as *present* the process that nearly always occurs is something as follows: first D itself, then the word "present," then some durating procession, most probably some sensation procession of our body or our surroundings at the particular moment. The length of this last associated durative procession is variable; in quick apperceiving, as in quick remembering it may be little more than the word 'present' alone; or it may be the quick flash of some mental duration even without the word "present."

But while on this subject we must not let words confuse several distinct data. Strictly the *perceived Present* is the content of any perception at the time of its occurrence; is that occurrence itself. Similarly the *apperceived Present* is the occurring object of apperception; that which directs the association. But to apperceive *the* Present that is, to apperceive the mental content actually occurring *as* the Present, that is again to perceive its *relation* to the Present, this occurring content must call up, and be associated with, the idea "Present"; that is very likely with the word "present" followed by some durating procession very probable to be a continuation of the surrounding stream which was the object of apperception from the beginning; in other words the apperception of the Present *as* the Present is usually but a sustained association of the word "present" with the progressive flow of the sensations within us or from around us. The *change* which reveals the relation, that is, the change which

constitutes the relation, in so far as a relation is a psychological occurrence, is the change occasioned by the dawning appearance of the associated idea.

So much for the Present, for apperceiving the Present as the Present, and for apperceiving D as present; now for the 'goneness' of A, B or C — the Past. Strictly speaking, mental content has no Past and no Future; they are only while they are, and their existence is like a mathematically moving point, or speaking of the total content, is like a plane moving at right angles to itself. What then is this moving procession by which, as we say, we have knowledge of the Past?

From what we have discovered regarding Present, we may suspect that perception of Past, perception of past relation or relationship, apperception of Past, and apperception of past relationship are all different matters. As the existence of any temporal portion of any mental content constitutes a perception of Present, so the *cessation* of its existence, constitutes a perception of Past. In order to perceive Past, some sensation or image must *cease*; whenever any such ceases, we perceive Past; the ceasing of the perception is the perception of Past; did no perception ever cease we should never perceive or know anything whatever regarding Past, or pastness, or about *the* Past.

To have a perception of past relation, a *relation*, that is, as we have explained, a *change*, must occur. To perceive a temporal *relation* between A and B, B must be different from A, and to perceive the relationship the relationship must occur; and we shall perceive whatever occurs, and we shall perceive it as it occurs, while it occurs, and in its occurrence; and we shall *only* perceive what occurs and while it occurs, and in its occurrence. To perceive A before B, A must occur and B succeed. To perceive B after A, A must occur and B succeed. The *perceptions* of the relation, A before B and B after A, are identical because the relationship A before B is the relationship B after A. The *apperceptions* described by these two phrases we shall discover may be quite different.

For an apperception of Past, the cessation of some sensation or image must call up some *idea* of Past, of something ceasing; striking or familiar examples, those which most for-

cibly impress memory, are those most *likely* to be called up; yet the least possible flitting perception of something ceasing would suffice for the associated idea; or even merely some word, such as "past," "gone," etc.

When we come to apperception of past relationships, we arrive upon confusing and difficult ground; not because the essential and typical process is different from all other apperception, but because the associated ideas are so varied in number and kind, and our uses of language so loose and delusive. First we must note that an apperception of Past is not an apperception of past relationship. For example, A may occupy the focus of attention and its cessation call up associations of ideas of pastness. In this case B did not occur at all, and in the associations brought up, the pictures are of single terms ceasing. This is apperception of Past. But to apperceive any temporal *relation* A B, the change A B must occupy the focus of attention and its occurrence call up by association some *idea of relation*; that is, some mental picture of change, some *a* followed by some *b*.

Our space will not allow us to analyse all the apperceptions of temporal relationships of past, nor is it necessary to do so; a few important types will give the key to all. Perhaps the most crucial in the whole time problem is that which takes place, when, as we say, we perceive that something is *of* the Past; a moment *ago* I knocked on my table so hard that it hurt; I heard the knock and then felt the pain. What in my mental process constitutes this "ago"? Clearly the "ago" is a relationship with some present. But what sort of relationship? a perceived or an apperceived one? And with *what* present? Is it with "now-now-now-now"? Or are we to speak of some particular "now," that "*now*" is not "now" at all, but as we shall see a mere idea of "now." When I felt the pain, I was not *thinking* about its time relations, that is I was not apperceiving such. I did *perceive* the time relation of the sound to the pain; I did not *apperceive* it. It is quite possible that a representation of that sound may pop into my head again immediately after actually hearing a similar sound; I shall then perceive a time relation between that representation and that sound, but I shall not necessarily *ap-*

perceive any time relation between them; *whether I do or not will depend on whether the occurrence determines the subsequent current of association; and the kind of relation that may be apperceived will depend upon the kind of associations that are called up.* Suppose I do hear a similar sound at some future time of my life, and by some favorable condition determined by my surroundings or thoughts at that time, a representation of the former sound and of all its surroundings at its time of occurrence be by association again brought into the focus of apperception; that having thus sprung into mind *by* association they should then dominate and determine by association the next and following course of apperception and so on. What takes place here is a present sound like a former sound, followed by a representation that is like it and also like a former sound, followed in turn by a panoramic representation of that certain stretch of my past life that happened when I struck the table, heard the original sound, felt the pain and so on. Thus far there is in all this no apperceived "ago," no apperceived time relation, merely this panoramic representation of the Past is passing through my mind; I have not yet apperceived or, as we say in ordinary language, I have not yet *recognized* that it *is* the Past; no least thought of the temporal *relation* of all this panorama nor of any part of it, not even of the represented knock, to the Present may have yet occurred to me. What shall we say so long as this panorama goes on and no direct time-relation to the Present is thought of? Shall we say all this is nothing but imagination? This question I think brings us to one of the most usual sources of confusion for our entire subject. Usually we *do* call just such panoramas as this memories, and remembrance, whether we do actually stamp their date upon them and think their "ago," or their "how long ago" or not. The vast majority of the representations of those things which have happened but a moment or a few moments before, we have no need to date and do not date. The stream of thought or apperception into which they rise is not one regarding time relations or time characteristics, or time recognition. For instance, had I been writing an explanation of pain instead of time, the same panorama of the table, my

hand moving toward it, the thump, the pain, would have occurred as now when I write of time; but from this point instead of a train of associations of time nature being set going, a train of pain relations would have been set going; that is I should have apperceived pain and not time. The mere passage of past panoramas through the mind in no way constitutes a *recognition* that they are of the Past, or of how long they are past. Presently I shall show the difference between imagination and this sort of undated, unrecognized memory; we are now examining dated memories, and we wish to know in what this dating consists over and above the mere passing picture. From what we have discovered, this should be comparatively well understood. First regarding the knock we may merely *think of it past*, without bringing in the Present or any particular time relation; that is we may merely *apperceive it past*: in this case its image or representation will merely bring up in apperceptive process, ideas of Past; the image of the thump will cease and ceasing images will follow; perhaps the representation of the knock will continue to occupy the centre of the stage for some time, will continually go through the process of ceasing and of setting associated images to ceasing; and for the time the whole play will be a regular variety performance of ceasing, while we may or may not be saying all the time or repeating all the while to ourselves the *words* 'past, past, past'; or 'thump past,' 'thump past'; or if we had been less engrossed with the particular performers, any portion of it might have sufficed; a single "tumble" or cessation of the first comedian Thump himself, followed by a tumble or two of the associated company; or if even less engrossed, a mere glimpse of Thump followed by the word "past" would have completed the theatrical bill. This is the simplest form of apperception of the relation Past; the *change* is that from the ceasing thump representation, to the associated ceasing representations; the *pastness* lies in the relational change to the associated idea of ceasing, and this idea is composed of the associated ceasing representations.

Next we may apperceive that the thump happened *before* the pain; here 'Thump then Pain,' 'Thump then Pain' will be the chief theatrical performance, in imitation of the origi-

nal actual occurrence; if we are not in a critical mood this pantomime may suffice even without the word "before"; the *repetition* of the main performance may constitute the idea; or if we are more reflective and exacting, the whole company may be called out, and the whole stage be set whirling with mimic and peek-a-boo representations of 'beforeness.'

The "show," for apperception of the fact that the pain happened *after* the thump, would differ little from the last above. Pain here would come on to the stage first, making the bow, as it were, that introduces the "show," instead of Thump, as previously, and he will probably make an extra bow between each alternating bout between him and Thump, just to make sure that we keep our eye mainly on him; and every time he makes such a bow he (or we) will say "after," or to speak more soberly, the word "after" will say itself by association, instead of the word "before" saying itself.

The play by which this Thump-Pain representation is apperceived, *i. e.* thought of in relation to the moving Present, may now be easily understood; we here no longer look alone at the stage, but we take in the whole view around us, from our body outward and, as well, from our body inward. Thump-Pain are the chief actors on *the inner stage* as before; they are the *first* objects of apperception from which the course of thoughts wanders momentarily down among the audience, that is to our actual 'now-now-now' surroundings, and inward to our own bodily sensations and even to attention to our own thoughts; but now and anon our focus of attention flits back from these actual Presents to the show 'Thump-Pain,' again viewed on the mimic stage of memory. And as we have said of simple apperception of Past, so of this process of apperception of 'having happened before the Present.' Here the play may be longer or shorter according as we are more or less reflective; a twinge of neuralgia may suffice for the moving Present, with or even without the word "present" or "now"; and a single bow from Thump or Pain, that is a single memory image of these may suffice for their remembrance, or there may follow a full apperception of 'Thump-Pain past,' that is of the *ceasing* of the thump and of the pain, as described above.

We have now described what takes place when, as we say, we think of a thing or event as past, and when, as we say, we think of something as *of* the Past; that is, past with reference to the moving present. But particular time-relations, such as yesterday, last week, a year ago, ten minutes since, remain to be discussed. But as this brings up the subject of *measured* time, let us postpone these for a word concerning so-called perceptions of Future. As the fundamental sign of every idea of Present is the *continuation*, and that of every idea of Past is the *cessation* of some representing image, so the fundamental sign or characteristic of every idea of the Future is the *beginning* of the representing associated images. When I think of the Future of things, I think of them as *beginning*. As I go over a familiar way, memories of the path ahead of me beyond my view keep *rising* in my mind and constitute the foundation of expectation. If I apperceive these expectations, as expectations, the associations are those of the *act* of expectation, plus the panoramas of the path. In this case, *I* enter into the "show," the whole moving action of my bodily feelings while I sit here or walk there and expect; that is, certain holdings of the head, wrinkling of the brows, laying my finger to my chin, or the like; meanwhile the stage show goes on, the performances now being emphatically those of the "beginning" nature or plot, together with little mimic side pantomimes of myself in the acts and experiences of expecting; also the orchestra plays "future" "future" the while, or anon, plays "expectation" "expectation," and the panorama of the path ahead of me moves on in ever beginning glimpses. Apperception of Future, and apperception of *the* future, are similar to the apperception of expectation, and, I think, need no further explanation than may be derived from the above.

But how do we *measure* time length, and measure "how *long* ago," and "how long until?" When speaking of our simple creature capable of but a single constant sensation, we said that when his pain lasted five seconds, he perceived the length of five seconds, and when it lasted one second, he perceived the length of one second. We distinctly declared he did not apperceive either length, and from what we have said

of change and relations it is clear that I have not conceived that this creature perceived relations of any kind; neither relation of difference nor of number. Here we must be most careful not to let our customary use of language and our common processes of thought designated by common language, confuse us as to the actual elementary processes of mind which we never experience singly, and for which consequently we have no common or definite designations; and, what is more usual, have no definite or apperceived conceptions. Until some one opened our understanding to the matter, we went on deludedly imagining that we saw distance through rod-and-cone processes, the same as we did blueness; now we discover that what we call *seeing* distance is chiefly not seeing at all. It is probably the same with all the ultimate elements of sensation; Prof. Wundt reminds us that we never experience them singly, and so with great difficulty arrive at any conception of what each or any one element singly of itself is like, or what its various attributes are like. We should be prepared therefore to comprehend, since apperception of length and of number is not perception of length or of number, and again since perception of *difference* of length, and difference of number, (these all involving changes and relations) are not mere plain perception of length and perception of number, we should, we repeat, yet be prepared to comprehend that perception of five-seconds length is not in ultimate nature the same as perception of one second length. That there is a difference here, we think it comparatively easy to demonstrate, though it is quite certain that we do not ordinarily apperceive the difference, that is, do not form and associate ideas of it with its occurrence. It is probable, in our ordinary apperceptions of time length, that the associated *ideas of length*, which make up the apperception, are those representations or memories of muscular tensions, dermal stretchings or joint pullings, which fundamentally are the components of our ideas of motion; consequently has perception of time been so commonly founded on perception of motion, from Aristotle down to present psychology. There is little doubt that the intensive changes, which are the characacteristics of these motion sensations, are the striking and

characteristic components of those associated ideas which enter into our ordinary apperceptions of time length. But we must not fail to note that these changes are not the only components of these ideas, and that these image processions, and also their prototype original processions, are not *all* change ; there must be duration without change in order for duration with change to be possible. And in the same way that we continually *perceive* changes different in degree of change, without *apperceiving* any difference, so it is probable, and I think certain, that we continually perceive durations of different lengths without apperceiving their difference. For example, of our simple creature I think one should now have no difficulty in conceiving how there might and would be a difference between his perception of a five-second pain and his subsequent perception of a one-second pain, and yet this creature never perceive the difference ; that is, might not have any relational idea of such a change, as we might find to constitute the process of perceiving or apperceiving *difference*.

Prepared, therefore, not to confound actual difference with perception of difference, let us examine these matters more closely. We found that duration and change are ultimate data ; we shall also discover that differences of duration are also ultimate facts. We shall never discover why ultimately these differences are differences, but given these differences, we shall discover, I think, how we come to perceive, and finally to apperceive, these differences, and in what these processes consist. Carefully considering the matter in the light of the experiments reported in Chapter III, I have been led to suspect that this perception and apperception of durative differences may rise in two ways, which, for convenience, I shall here designate as the single method, and as the multiple method. These experiments emphasized the fact long before determined, that our so-called memory images are dependent upon certain reproductive *habit* processes of our nervous and bodily organism. Were it not for these "habits" we should have no memory. My experiments emphasized *the degree to which the validity of correlation between these so-called memories and their originals, depends upon the validity of these organic habit processes.* If the habit is not accurate,

the memory will not be faithful, *although we shall not have the least suspicion that it is not faithful.* The truth is, the memory may be altogether different in temporal length from the original temporal length without our perceiving or recognizing their difference, or suspecting anything about such a difference whatever. Nothing can bring out more clearly than this, that *actual* difference does not constitute recognition of difference, and that perception and apperception and recognition of difference are all some sort of processes quite different from and additional to mere actual difference of occurrence. *To apperceive these differences, they must, by association, bring up certain qualitative ideas and ideas of difference.*

We do not yet know positively the particular portion of the brain organism, whose rhythmic reiterative habits are chiefly responsible for memory; it is sufficient, however, for our present purposes that it is *some* particular portion of nerve organization, which, for convenience, I shall here designate in accordance with present probabilities, as the central nerve cells. My experiments demonstrate that when these cells functionate with reiterative temporal accuracy, our time judgments are accurate, and as their habit varies or is disturbed, our judgments vary correspondingly. We have also to observe how frequency and lateness of original occurrence form and influence this iterative habit. We have then to note, that immediately after the occurrence of a definite sensation, which previously has been frequently repeated, say the tick of a metronome, two forces, or to speak more accurately, the tendencies of two processes, are contending against each other in the production of the succeeding memories; and, indeed, as well in the production of the succeeding *sensations* themselves. The cells, both those which functionate the memories and those which functionate the tick sensation, (be they the same or not, we do not know) tend on the one hand to follow the rhythm to which they have previously been trained, tuned or accustomed, and on the other hand, to adopt a new rhythm in correspondence to the rythmic impulse then and there received from the metronome. Not only, therefore, is the result likely to be ever a compromise between the two, and our sensations at different times and under various conditions, likely to vary

from the actual metronome rhythm and from each other, but quite possibly another result of more peculiar nature may also happen from and during this contention of tendencies. For instance, suppose the metronome to be beating quarter seconds and the cells to have been tuned or adjusted by preceding practice according to the method of our experiments to second beats. Plainly by the law of association and habit, the first stroke of the metronome sets going the tendency of the cells to perform their second-beat representations; and consequently the impulses sent in from the succeeding second, third and fourth beats of the metronome will find the cells in a different condition than did the first beat. Precisely what would be the nature of the result of this contention or disturbance of the regular order of things, or what the difference between this and the case where the old habits of the cells should be entirely overcome by the new influence, or where the cells from the beginning were accurately adjusted to the beat coming from the metronome, is difficult to say. It is well to note, however, that this condition of contention between new and old influences or habits is the usual condition rather than the exception; and that any peculiarity of sensation or feeling which should result, as is very likely to result from such a struggle, might be a very important factor in time measurement. Not that such a peculiarity or temporal sign would of itself alone constitute *apperception* of time length, but reproduced representations, or repetitions of these different temporal signs among the associations constituting the apperceptive after-train of ideas called up by *actual* time differences, may be definite and determining data in such apperceptions of different time lengths. And in consequence of these contentions also, and of apperceptions which they determine, it is quite possible that in the *original occurrence* of familiar sensations we may have indefinite cognizance of "too short" or "too long" *without definite memory or apperception of that in relation to which it is short or long;* it is quite possible that these definite memories sometimes are and sometimes are not then called up by these apperceived signs. In short, during the original occurrence of a series we may, as it were, apperceive a general abstract definiteness of length or of time difference or

relation, without its being followed by concrete definiteness; that is, we may apperceive that it is definite without apperceiving its full definiteness, for such subtle tricks are, by no means, psychically uncommon. Should that which we have tried to describe be true, those theories which have sought to explain time relations and time perceptions by "temporal signs" or a disparate sense would have herein some foundation of analogy.

But more frequently perhaps are the rudiments of time measurements to be discovered in a method different from the above. Should an image occur simultaneously with its corresponding sensation, the two beginning and ending precisely together, this equality of their length, would, in accordance with our foregoing nomenclature, constitute the perception of their equal length. Without fuller description, we may understand how by association this perception would rise to apperception, and thence to apperception of their temporal equality. Similarly, if the image and sensation were of unequal length, we may comprehend how this would rise to apperception of their inequality. Again, if equal temporal series of simultaneous sensations and memories, or yet again, unequal temporal series of such, occur, we may also prefigure how these get apperceived, and what will constitute the nature of such apperceptive processes. But before we speak finally of such processes, a word must be said as to apperception of number, in order fully to elucidate how we apperceive a sensation to denote *so many units*, or to be so many times longer than another.

For four sensations to be perceived, four sensations must occur; for these to be apperceived, the idea of four, *i. e.*, the word "four," or some four image reproductions, or both the word and the four reproductions must be added in proper apperceptive process thereto. So of any other number of sensations or images. This is the key to the simple apperception of number. A sensation, four seconds long, may occur succeeded by four different sensations, each one second long; by our first method of measuring time length, combined with the apperceptive process of number, we may understand how we arrive at an apperception of one sensation being four times the length of another. Or a sensation four seconds long may occur

simultaneously with four sensations, each one second long; and so by the second method of time measurement, combined with the apperceptive process of number, these would rise to apperception of the one as four times the length of the other. And so on with other multiple number-measurings.

Before leaving finally this subject of habit rhythm and time measurement, a word more regarding those theories which have found in our main unconscious bodily rhythms, such as breathing, pulse-beat, and leg-swing, standard rhythmic measures of our time judgments. We have pointed out as objections to these theories, that we have no reason to conceive why one such unconscious process should dominate as a standard more than any other; yet for all to contribute such unconscious disturbances would, indeed, so we must think, lead rather to indiscriminate confusion, than to standard discrimination; such views, moreover, run quite contrary to the selective advantages of unconscious reflex actions, which, by relieving consciousness of all such disturbing vital processes, have made our conscious processes distinct and intelligible. Also we have mentioned that, according to the theories of breathing standards and the like, it would seem that we ought to have a more lively and accurate conception of the definite length of such processes as breathing than of any other duration lengths or rhythms, while, as a notorious fact, we do not; but rather those rhythms which we most customarily hear are those which most vividly rise up with accuracy and as standards. This brings us to the point on which we wish to lay further emphasis; and for this we would note that the particular function, to which our conscious centres seem to be differentiated in contradistinction from the reflex unconscious centres upon which our vital processes depend, lies in just their power and tendency *to adapt* themselves to the multitudinously time varied outer impulses to which consciousness is to correspond, and whose purpose it is to represent; their very peculiarity consists in differentiation to outward susceptibility rather than like the unconscious reflex vital nerve centres to a particular inward rhythm approximately undisturbed by outer influences. Nor must the fact shown by our experiments, that unusual frequency of repetition by the *conscious* cells of im-

pulses received from without tends to perpetuate such particular time rhythms or habits to the temporary detriment of accurate judgments of other rhythms or time lengths received from without, be counted against this view, but rather for it; for if there were *no* tendency for these conscious cells accurately top reserve their habit of repeating the occurrences from the outside, which were their original prototypes, there would never be any accurate time memories or images of our sensations at all, in fact, no rational memory whatever. The whole cerebral and central nervous organism seems a happy adjustment of fixity of habit, not too fixed, and susceptibility, not too susceptible. There would seem reason from *à priori* grounds to suspect, therefore, that which from observation seems to be the case, that our standards of time-measurements are memories of certain most striking rhythmical, habit-inducing, and oft-occurring outer occurrences, such as the particular length of watch or clock ticks, which we are most accustomed to hear; the sounding-hours; the varying lights and shadows of morning, noon and night; the peculiar Sundayness of Sunday and Mondayness of Monday; the varying seasons; perhaps also as we have surmised vague temporal signs or admonitions of passing moments and as well of passing years.

After all the foregoing, it seems unnecessary particularly to explain apperception of such time relations as "yesterday," "to-morrow," "last week," "a week hence," "a year ago," or "ten minutes ago;" these terms are but particular words associated with particular time occurrences and number measurements, which rise into more or less extended and definite processes of apperception of such relations, according to our reflectiveness of mood or passing mental circumstances.

We have seen that much of our thinking is comprised of image-trains representing past occurrences to which we attach no date; which we do not think of or apperceive as of the Past at all; that is, which we do not actually *recognize* as of the Past or as ever having been seen before. We have to repeat that in our belief some of the chief confusions of psychology, and as well of philosophy, come from commonly mistaking this mere passage before us of trains that *are* correspondent

to former trains for those mental processes which do properly constitute psychological recognition. It is curious to note that those metaphysicians and psychologists, who most stickle against the possibility of any *real* recognition of any non-psychical *real* world, most unsuspiciously build their systems upon fancied *real* recognitions of past sensations in so-called present representations of such. The truth is that in the absolute sense we do not any more recognize sensations in their image representations than we recognize real things in their sensational representations. Until it dawned upon the human mind that its former so-called recognitions of an outer world could all be explained without the real existence of such a world, no one suspected the *reality* and *validity* of these recognitions; we now all admit such so-called recognitions to be but psychic processes; the *validity* of these processes and recognitions is, and we think must for a long time be, a subject of debate. We here wish only to point out that these parallel recognitions, so-called, of former sensations are likewise but psychic processes, the validity of which is as much open to suspicion, as inferred and as hypothetical as that of the so-called recognitions of a real world, and, indeed, vastly more so; for how commonly are our most confident memories mistaken, and our insane and hypnotic subjects engulfed in hallucination.

Still more is this truth forced upon us when we comprehend the details of these processes of so-called recognition; when we clearly understand the psychological difference between imagination and so-called recognition. If every one of us through life were but rational every alternate minute and insane turn and turn about every other minute, there would be no difference between imagination and reality. The grounds for our present belief in some real difference lies in the constancy of our belief itself, and when we come to examine into it, we find this belief is a hypothesis, an inference, and no *positive* knowledge. But what then are the grounds for this hypothesis? Plainly not in any simple direct cognitive act or state. We have sure reason to believe that our ordinary so-called perception of time relation is not a peculiar disparate state, but an apperceptive process; and similar analysis, I think, discloses to us that recognition is a similar appercep-

tive process, and that imagination is still another such a process. The difference between imagination and recognition lies first in a marked difference in the character of the thoughts which form the objective process of the apperceptions, that is, to which the associated ideas are added in the two cases; and, secondly, in the character of these *added* or associated ideas or processes. And as it is the nature of apperceived associations to be of like character to the objects of apperception, we shall find that the difference between the associated ideas in our two cases corresponds in characteristics to the original difference between the objective processes themselves. What then is this fundamental difference between imagination and reality? We can only answer with an hypothesis, and this hypothesis is, that all things *do* occur in a fixed order, that all occurrence *is* a fixed order; that not all this occurrence is perceived by us; that certain of the total occurrences of the universe result in fixed and definite influences upon our brain organism; that like causes produce like results; that like stimulations produce like sensations; that like series of stimulations are followed by like series of sensations; that these physical stimulations *are* alike and these corresponding sensations *are* alike, though the mere occurrence of their likeness by no means constitutes our recognition of this likeness; that owing to the peculiar nature of our physical organism and particularly of our central nervous organism, whereby physical processes tend to repeat themselves, certain representations or repetitions of sensations corresponding to these processes in certain characteristics *do* tend to occur whenever these physical processes *do* repeat themselves; that the accuracy and scope of complexity of temporal correspondence between these representative processes and their originals depends entirely upon the habit validity of these physical reiterative processes; that our recognition of this validity and correspondence does not consist in some super-added cognitive act over and above those psychic processes which correspond to these reiterated physical processes, *but is entirely dependent upon, and to be explained by, a hypothetically actual correspondence or likeness of these reiterative processes, both phy-*

sical and psychical, to former processes, physical and psychical; finally, and again, that not even the mere validity of this correspondence alone comprises "recognition," but that recognition is a psychological process, the validity of which rests upon the validity of such correspondence. Our hypothesis is that the events of our lives do happen in a single definite actual order, which so impresses itself upon our memory organism, that by proper associative incitement, this order tends actually to be repeated. It is true that this same memory organism, lacking these major associative incitements, forms secondary associations, and these tertiary, and so on almost to infinity; and in proportion to the frequency in which these minor associations occur, and in proportion to their kinship to original occurrences, do they also tend to rise in association processes. These minor and less constant associations are the basis of imagination; "imagination" is a word which we associate with these *inconstant* flights of association; "reality," "actual," are words we associate with the main *constantly* reappearing stream of association. The fundamental difference between imagination and recognition lies in the fact that the iterative habit of our nervous organism is so susceptible to original *outer* influences and so accurate and persistent in repeating these, that they ever *do* remain a comparatively unbroken series in representation, while those series which happen not by any outward actual order of incitement, but by secondary associations of portions of those primary series, do *not* persist in like unbroken representation. If, by any chance, a new link can be fastened into the original or actual memory order with the same associative firmness and strength as an actual occurrence would have been, then such will actually appear to be recognized as actually having occurred and psychologically will be so "recognized." Liars who frequently, actively and consistently enough practice their imaginary associations, do eventually arrive at such psychological "recognitions;" all of us at times suffer such hallucinatory remembrances, and actually believe we did so, or so, or that such and such happened, when, actually, they did not; and the hallucinations of the insane and the hypnotic are

confirmative of our hypothesis. Imagination is inconstant memory; remembrance is constant memory. As we have said both these processes commonly go on without conscious recognition of the fact that we *are imagining* or *are recognizing*. When these last processes occur, simply the bodily act rises to the focus of apperception; and in apperceiving the "act of imagination," imaginative ideas, that is, inconstant memories are called up and flit before the mind; while in apperception of the "act of recognition," portions of the constant train of memory are called up to constitute the apperceptive association.

Let us summarize the foregoing: Our simple creature received series of like sensations, but he did not recognize them to be alike. So we, if incapable of memory, should experience often repeated sensations, but should never recognize them to be the same. Even, if endowed with memory, we should never recognize a constant actual series of life's events, did not life's events happen in a single definite order. Our actual remembrances are representations which *do* follow the actual order of original events. Our imaginations are representations which do *not* follow the original order. The validity of our imaginations and of our recognitions, depend alike and absolutely upon the degree of faithfulness with which the neural processes which produce them correspond to the neural processes which produced the original psychic events.

Briefly stated, the final result of this protracted investigation of the time problem is as follows:

The general consensus of past and of current opinion is that time perception must alone be accounted for within some peculiar simultaneous psychic state, and, according to most authors, by some peculiar and disparate form of consciousness, in addition to our stream of ordinary sensations and their representative images.

The conclusion which we offer is that the processes of our environment, of our bodily organism, and of the sensations and images which correspond thereto, are, in themselves, within the limits of the insoluble mystery of the existence of

any physical or psychical existence at all, a sufficient explanation of time-psychology, and that time perception cannot be explained by any single state or disparate sense, but can alone be accounted for as a *process*. The bearing of the experiments of Section III upon these conclusions, and of the conclusions upon the experiments is obvious. The author is conscious that neither the one nor the other exhausts the topic, and will be content if they draw closer attention and study to the *habit relations* between neural and psychic PROCESSES.

Approved as a Thesis for the Degree of Doctor of Philosophy in Psychology at Clark University.

G. STANLEY HALL.

Worcester, Mass.,
Friday, May 1st, 1891.

PSYCHOLOGICAL LITERATURE.

I.—NERVOUS SYSTEM.

Report of six lectures on cerebral localization, delivered in Boston, by Dr. Henry H. Donaldson, before the Boston Medico-Psychological Society, February and March, 1891. From notes by T. L. Bolton.

It was the aim of these lectures to show the bearing of the more recent anatomical investigations on the question of cerebral localization, rather than to give a full account of the subject.

Lecture I.

Gentlemen:—I shall open this course with a statement of some recent advances in our knowledge concerning the structure of nerve cells and nerve fibers, and the relation of these to one another. The advance has come about by the introduction of a new method, which is due to the Italian histologist, Golgi.[1] To the labors of Golgi and his Spanish pupil, Ramon y Cajal,[2] is due the discovery of the most important points which are to be described. As everything depends upon the validity of the method employed, I will briefly state its essential character. The method of Golgi outlines the nerve cell and its prolongations by means of a deposit or precipitate which is formed just outside of these structures, and occupies the lymph space which surrounds them. The deposit is an inorganic precipitate of a silver or mercury salt, and forms a dense incrustation about the nerve elements. Further details are not necessary here. The result of this reaction is to outline the nerve elements in black on a light background. The inference is that, where this incrustation goes there goes a prolongation of the cell. On this assumption, which appears in the main well founded, depends the entire significance of the method. Golgi's first result was that the axis-cylinder process from the nerve cells was branched. Closer examination of axis-cylinder processes indicated that they might be grouped in two classes; first, those in which the branching was not sufficient to obscure the identity of the main prolongation; second, those in which the main prolongation divided into many branches soon after leaving the cell, and thus lost its identity and faded out. In the dorsal cornua and in the so-called sensory regions of the cerebral hemispheres, this second type of cells was found whereas the first type appeared in the ventral cornua of the cord, and in non-sensory portions of the cortex. From this general distribution, Golgi was led to designate the cells of the first type as motor and those of the second as sensory. To the axis-cylinder of the second type Ramon has added some very suggestive details in that he finds the smallest branches of these prolongations surrounding in certain cases cells, *e. g.*, the cells of Purkinje in the cerebellum, and enclosing them like a basket. This manner of termination of the ultimate branches of the axis-cylinder appears particularly well developed in the instance cited, but it furthermore appears to be the usual manner in which such branches end when they terminate in the neighborhood of nerve cells.

Besides the prolongations which come from nerve cells lying within

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1 Golgi: Sulla Fina Anatomia degli Organi Centrale del Sistema Nervoso, 1886.
2 A. Kölliker, Zeitschr f. Wissen, Zool. Band 49 u. 51.

the central nervous system, the dorsal cornua of the spinal cord are filled with a mesh-work of a similar character, which is due to a breaking up of the axis-cylinders of the dorsal roots. All the sensory fibers of the central system appear to come from the spinal ganglia or homologous structures,—the fibers of the optic nerve are alone the possible exceptions to this rule. It will be seen from this arrangement that the sensory cell of the older histologists—meaning, thereby, a cell situated in the dorsal cornua and sending an axis-cylinder through the dorsal nerve roots to the periphery—finds no place. It has, therefore, been thought best to modify the terminology so that by sensory cells are meant those forming the spinal ganglia and giving origin directly to the sensory fibers. Motor, or efferent, is the terms retained for Golgi's cells of the first type; but the cells of the second type, which he termed sensory, are perhaps better designated as central.

While our interest has been specially attracted to the axis-cylinder process, it may be well to point out that the protoplasmic prolongations of the nerve cells have been brought out by this method with unusual distinctness and detail, and since they cannot be seen to unite with one another nor to give rise to nerve fibers, the question has been raised concerning their function, and the general conclusion is that they must be looked upon as nutritive. I wish for a moment to leave the nerve cell and to take up some recent results relating to the histology of nerve fibers. We have in the medullated nerve fiber an axis-cylinder surrounded by a sheath of somewhat complex structure. The axis-cylinder is the portion of the fiber which interests us at this moment. There are, roughly speaking, two views concerning its structure. One which has been ably advocated by Nansen[1] looks upon the axis-cylinder as a mesh-work, in the cavities of which is to be found a plasma, and this plasma is the active substance in process of conduction. The second view, which has recently been elaborated by Boveri,[2] considers the axis-cylinders as made up of a number of fibrillæ floating in a plasma. These fibrillæ are considered as unbranched and as continuous from the nerve cell to the termination of the axis-cylinder process to which the cell gives rise. If we look upon the axis-cylinder prolongation as made up of these fibrillæ, then the branching of the axis-cylinder is to be interpreted as the giving off of small bundles of fibrillæ. In considering the manner in which the nerve fibers arise from the nerve cell, I may allude to the recent observations of His[3] on the development of the spinal ganglia in man, which show that the well-known T process of Ranvier, by which the cells of the spinal ganglia are joined with the nerve fiber, is a derived structure. These cells originate as bipolar nerve cells, similar to such as are found in lower vertebrates. In the course of development, however, the two poles from which the nerve fibers originate gradually approach and finally fuse with one another, thus giving rise to the stem of the T, one branch of which runs centrally and the other toward the periphery. In the hands of Ramon the methods of Golgi applied to fœtal, or very young animals, in which the nerve fibers were only in part medullated, has developed some startling results concerning the branches of nerve fibers. The net work which Golgi observed in the dorsal cornua of the spinal cord was the final termination of the dorsal root fibers. Ramon has now shown that as soon as the fiber enters the cord it divides into two branches, one running cephalad, the other caudad. That these two branches give off further, branches at right angles to their course, which have been called collateral fibers, and that it is the termination of these collaterals which gives use to the mesh-work already men-

1 The structure of the Histological Elements of the Central Nervous System. Bergen, 1887.
2 Abhandl. d. k. bayer, d. Akademie. Wissen, Band XV, 1885.
3 Archiv. f. Anat. u. Physiol., 1890.

tioned. These collaterals, however, are not confined to this group of fibers alone, but, as it appears, may be found arising from fibers in almost any part of the cord. This rather startling result is difficult to explain, if we consider that the medullary sheath of the medullated fibers in the cord is unbroken throughout its extent. The recent observations of Porter[1] show, however, that there are nodes of Ranvier in these fibers in the spinal cord, and, although we know nothing of these nodes further than their probable existence, they would seem to offer a convenient point of departure for the collateral fibers and thus bring the law of branching in the central system into harmony with that for the peripheral nerves, where the branches occur at a node of Ranvier.

There is one principal point which has thus far been left out of the discussion, viz.: how far are the fibers which are brought out by this method of Golgi to be identified with the medullated fibers with which we are commonly familiar? It seems beyond doubt that a number of the structures thus developed are unmedullated even in the adult. One important piece of evidence has been presented by Flechsig[2]. He has succeeded in staining specimens of nerve cells, which had been previously treated by Golgi's method, with a dye stuff which stained medullary substance red. According to this result he finds many of the branches of the axis-cylinder process to be medullated, and thus it would appear that these branches may become medullated nerve fibers.

We come now to the final point of the connection between nerve cells and nerve fibers. Of course each nerve fiber is looked upon as the outgrowth of a certain nerve cell, and the connection referred to in this instance is that between the termination of a nerve fiber and neighboring nerve cells with which it may be supposed to be physiologically associated. The connection of the nerve cells with one another is but another aspect of the same problem. It may be stated as a general result that this method fails to show any direct continuity between any prolongations of one nerve cell and those of another. This lack of demonstrable continuity has led to several hypotheses; the best of which is perhaps that of His, who points out that, however closely the protoplasmic prolongations may be interwoven, there is always a somewhat between them which we are accustomed to designate as ground substance, and to this ground substance must be attributed the function of establishing continuity between the nervous elements. The earlier workers in this line had directed their attention to the remarkable branches of the axis-cylinder prolongations, and thought in some way these would account for the physiological connections between cells. At present, however, there is no positive evidence in favor of such a view.

Taking a general view of the nervous system, we find the sensory impulse coming in through the dorsal roots which form a mesh work in all probability connecting, as a rule, with some of the central cells, and thus finding its way to the higher centers, or perhaps without the intermediation of central cells, reaching the efferent cells in the ventral cornua. Nansen, who wrote a few years since and was much impressed with the possibilities of the network formed by the prolongations of the axis-cylinder, drew up a scheme which I believe has been received with some favor, according to which the nervous and mental process were considered as taking place in this mesh work, while the cells were regarded as having a nutritive function and acting as the supporters of the mesh work alone. It now remains to point out some of the peculiarities of our point of view as determined by these results. I will give them in briefest form posssible. The new methods show that the axis-cylinder is branched; that there are several types of axis-cylinders; that these branches may become medullated, hence several fibers may

1 Quart. Journ. Micros. Sci. Feb. 1890.
2 Archiv. f. Anat. u. Physiol. 1889.

arise from one cell; that there are no sensory cells in the central system; that the axis cylinders are made up of fibrillæ; that nodes of Ranvier occur within the central system; that collateral fibers arise from the longitudinal fibers of the spinal cord; that no nerve fibers come from the proto-plasmic process; and that there is no continuity between cell elements in the central system, but that probably physiological connection is dependent upon peculiarities of the ground substance which surrounds them all.

Lecture II.

Gentlemen: This lecture will be a continuation of the preceding upon the architecture of the nervous system. I shall consider the size of the nervous elements, their numerical relations, the relation of the cerebral cortex to the optic thalamus, and say a few words upon some methods of ascertaining the localization of functions in the central nervous system. Nervous elements differ much in size. In speaking of the size of the nervous elements, one point should be emphasized. Nerve cells and fibers are in reality but parts of the same structure; the fiber is a branch of the cell. The nervous system must, therefore, be considered as composed of one kind of elements—the cells. There is a relation between the size of the cells and that of the fibers, more especially between the nucleus of the cells and the fibers.[1] The nerve fiber is composed of a sheath and an axis-cylinder. This axis-cylinder is made up of a number of minute fibers known as fibrillæ. The branching of a nerve is simply the separation from the main axis of a number of fibrillæ surrounded by a continuation of the sheath. All nerves terminate finally in a mesh-work of the fibrillæ. If we examine the ischiadic nerve of a frog, we find it to be conical in shape. Transverse sections of the nerve at the hip, the knee and the ankle, show a diminution in the diameter of the individual nerve fibers, as we proceed from hip to ankle. The old explanation for this was that the fibers themselves were conical in shape; but this is probably incorrect. The diminution in diameter is really due to the fact that the nerve trunk branches and the branches from the higher levels contain the fibers of greatest diameter, and those from lower levels contain those of smaller diameter. The physiological bearing of this is important. Suppose the proximal muscles of a limb to be as richly supplied with nerve fibrillæ as the distal muscles are, then since we have found that nerve fibers of large diameter supply the proximal muscles, it follows that in order to have the same abundance of fibrillæ for the distal muscles, that the individual nerve fibers supplying them must be more numerous. Here let us depart from the main point for a moment in order to bring up a subject necessary to make clear the explanation that follows. Every motor cell, so far as is known, acts as a unite and give rise to but a single motor fiber. Thus, whenever a discharge occurs in the cells, the muscle in which the fiber terminates must respond. If, however, a muscle be the termination of several fibers, several cells control its action and thus a finer control is brought about. The significance of size of nerve fibers is usually stated to be that the larger fibers run the longer course, for larger fibers are necessary to carry the nervous impulses to greater distances, on the analogy of electricity where the larger wire is the better conductor. The largest fibers in the frog arise from the lumbar region of the spinal cord and terminate in proximal muscles. The differentiation of function is slighest here, and fineness of adjustment is least needed. From this we conclude that large fibers are concerned with coarse adjustments, and fine fibers with fine adjustments. We turn now to consider what is known of the

1 Mason; Journ. of Nervous and Mental Disease, 1880,-81,-82.

numerical relations within the central nervous system. Birge,[1] working with Gaule, undertook an actual count of the number of the fibers entering the spinal cord by the anterior and posterior nerve roots. This actual number of motor fibers determined by the count corresponded very closely with the number of cells in the ventral cornua of the spinal cord. From this a munerical equivalence between motor cells and motor fibers was inferred. Gaule[1] has carried the investigation a step further by counting the number of nerve fibers in cross sections of the spinal cord at five different levels and has then attempted to determine whether there was any relation between the number of fibers of these various levels and the number of the dorsal root fibers entering the cord at the same levels. He finds as a result that each root fiber calls for eleven fibers in the cross section. Thus there appears to be a distinct numerical relation in this instance. It should be remembered that Gaule is dealing with medullated fibers only and whatever relations may be dependent upon unmedullated fibers do not enter into his calculations.

In his paper, upon this subject, Gaule has a system of philosophy which is peculiarly his own.

It may be roughly stated as follows: As the cells are composed of molecules which are made up of atoms standing in a fixed and definite relation to one another, so the body is composed of cells which appear in fixed proportion, for instance every sensory fiber demands eleven nerve fibers in a cross section of the cord.

Charts of the brain have been made by several authors for the purpose of showing the various developments of the cerebrum. Broca, Obersteiner, Eberstaller and Wilder have constructed such charts as you see, according, as it was their purpose to illustrate particular points. The need for some definite diagram on which to plot lesions useful for the localization of function has been felt. For this purpose the best of these perhaps is that of Eberstaller, which was designed to show almost every detail, while that of Wilder brings out the early developed characteristics mainly. The amount of variation that may occur in the central nervous system is very great. All brains differ. Several attempts have been made to measure the extent of the gray substance of the hemispheres. The figures vary between 1800—2700 sq. cm. The important relation is that the sunken gray matter lying in the sulci is about twice that which is exposed on the surface. Suppose that in brains which are comparable, the sulci have a similar depth and the nerve elements a similar size, then a richly convoluted brain would contain a greater expanse of gray matter, and a greater number of cells would be found in such a brain. The converse would be true; a poorly convoluted brain would contain a less expanse of gray matter and a less number of cells. If, as Gaule asserts, every nerve cell gives rise to a definite number of fibers and the amount of branching be similar in cases compared, that brain which contains the most cells, has the most branches. These branches become medullated fibers and constitute the white substance. According as the number of cells is greater, and hence the number of fibers large, the amount of white substance will be great and the size of the brain increased. However, Gaule's numerical relation is probably only partly true. All of us are acquainted with large brains that are poorly convoluted, and the preceding remarks on the relation of the size of the cerebrum to the abundances of their convolutions have for their main purpose to direct attention to the compensatory developments which probably occur there, but about which we know almost nothing.

1 Archiv. f. Anat. u. Physiol., 1882.
2 Abhandl. d. König. Sächs. Gesell. d. Wissens. B. XV, 1889.

Such a scheme as that of Gaule at once raises the question, how far variation may occur within the nervous system. We have there many decussations, such as that of the optic tracts, of the cranial nerves and of the pyramids. Variations in these decussations are known to all. Our physiological inferences are based upon anatomy. If the anatomical foundation can vary, it is a most important point, especially in the case of the nervous system, and one which must always be kept in mind, when physiology and anatomy appear to conflict.

It may be possible sometime to track a sensory nervous impulse from the periphery to the contex. All of us have been taught that sensory fibers enter the spinal cord by the dorsal roots, and proceed by the dorsal columns towards the brain, and terminates in the ganglia of these columns. From these ganglia they pass to the thalamus of the opposite side, by way of the lemniscus. The cells of the thalamus are connected with the cells of the cortex by the fibers of the corona. There is some reason to think that whatever the source of the sensory impulse, it must pass through the thalamus before going to the cortex. Several attempts have been made to determine the relation between the portions of the cerebral cortex and the thalamus.

Monakow[1] determined this relation in rabbits. He operated on the dorsal and lateral surface of the hemispheres only. Here the removal of definite portions of the cortex caused an atrophy in an equally definite portion of the thalamus. There were certain portions of the thalamsus, which were not affected by any of the lesions and these by exclusion may be supposed to be connected with those portions of the cortex which were never injured. These results have been in part verified for man.

LECTURE III.

Gentlemen: We shall consider in this lecture the motor regions of the brain. The middle portions of both hemispheres contain motor centres. A history of the subject is not needed; but the method used for the discovery of these centres and of the refinement of their subdivisions deserves some attention. Our especial interest is in the subdivisions. Motor is not a good term to apply to this region, but it is the best we have. The idea of the motor centres from an anatomical point of view is useful. The central nervous system may be considered as a conical mass with the cortical centres in the base of the cone, the apex of the cone representing the spinal cord. A nervous impulse proceeds from the periphery toward the cortical centres and at various levels in the cone it encounters masses of gray matter which increase the possible number of paths the impulse may take. The important question then is: Does the impulse diffuse itself throughout the entire system or follow a fixed path to a definite centre in the cortex? The possible paths depend upon the complication of the central system. In higher animals, the possibilities are many. The paths followed in any given case depend upon physiology rather than upon anatomy. The path of the impulse appears to be simple; it starts from a small area in the periphery and reaches a small area in the cortex. The cortical centre is but a specific point in the path of the impulse, where the impulse turns to pass centrifugally. As points in the path of an impulse the cortical centres are like innumerable other points in the central system, but they have a peculiar interest because of their great accessibility and because they are in a region which is supposed to be associated with mental phenomena.

Let us now consider the method of stimulating the cortical centres. Horsley[2] attempted to determine whether all the parts of the muscle

1 Arch. f. Psychiatrie B. XII, 1882.
2 Gotch and Horsley—Proc. Roy. Soc. London, 1888, XLV.

curve given by a muscle during an epileptic seizure, were due to the cortex alone or to cells lying outside of the cortex. He exposed the brain of a monkey and found the area for the control of the leg. An electric current was applied and an impulse sent to the spinal cord. By tapping the spinal cord in the dorsal portion and recording the impulses passing there in the pyramidal tract, by means of an electrometer, both the tonic and clonic portions of the curve were shown to be due to the impulse from the cortex. Others have observed that when the cortex was excised, and the stimulus applied directly to the nerve fibers, the clonic portion of the curve dropped out. This fact has been used clinically. Horsley has studied the minute representations in the cortex of movements of the head and limbs. His method was to apply to the cortex electrodes, two mm. apart, with a current just sufficiently strong to bring about a contraction. The strength of the current was important; a weak current would contract few muscles slightly and a stronger one would cause a stronger contraction of a greater number of muscles. To explain this contraction in the last case a slight irradiation of the stimulus was supposed to take place, so that neighboring centres were involved. The diagrams show the results of Horsley's experiments. The outline of the region enclosing the motor areas is largely bounded by fissures—below by the Sylvian fissure, behind by the inter-parietal, in front it passes somewhat in front of the praecentral, and above the margin of the hemisphere forms the boundary from this point of view. There is no necessary connection between areas and sulci; some areas appear to be limited by sulci, and others not. The portion of the cortex lying in the sulci is one that usually escapes stimulation.

In the motor region of the monkey's brain the motor areas for the control of the various parts of the body were found to be located thus: The head and eye area is located in the front part of the motor region; above the Sylvian fissure is an area for the control of the larynx, pharynx, and the movements of the mouth and face; back of the head area and above that for the face is an area for the upper limb; still back and above this is an area for the control of the lower limb; and between the areas for the upper and lower limbs is one for the trunk. It will be noticed that we thus pass in serial order through the centres for the head, arm, trunk and leg, the first most anterior and the last most posterior. The same serial arrangement is maintained on the mesal surface. There is a remarkable independence between the size of the centres and the mucles they control. The centres controlling the head and face constitute about half the motor region. Where the muscles are large and the movements crude, the representation in the cortex is small. The area for the head has been longest undergoing differentiation; next to this in order of development is that for the upper limb, that for the lower limb, and lastly for the trunk. Beevor and Horsley[1] have studied in detail the anatomy of these areas, especially that of the arm, and have found there is a subdivision of function within them. They divided the area of the arm into squares of 2 mm. on a side and stimulated these squares in regular order. Attention was given to what movements came out first—the so-called primary movements. The first movement following a given stimulus in the uppermost part of the arm area was the movement of the shoulder; when the stimulus was applied a little lower down, a movement of the elbow took place; it was then applied further down still, and a movement of the wrist was the result; when however, the stimulus was applied to the lowermost part of the arm area, the thumb responded. The centres for the control of the shoulder and the thumb are then farthest separated. The thumb is the most highly modified portions of the upper limb, and its movement is

1 Beevor and Horsley—Phil. Trans. Roy. Soc. London, 1887, 1888.

the most highly modified movement. The opposition of the thumb is very widely represented. It was determined that in general the march of a spasm affecting the arm follows the order of the centres within the arm area; that is, if the spasm starts at the shoulder, it passes by regular progression to the fingers, or if it starts in the thumb, it passes in regular progression along the limb in the opposite direction. It appears, however, that the connection between the thumb and shoulder is very slight, so that a spasm starting in the shoulder does not usually terminate in the thumb, nor does one commencing in the thumb, terminate in the shoulder. Looked at from one point of view this would appear to indicate that the association tracts in such an area were short, and, as a rule did not extend beyond the nearest lying centre.

No centre will give a particular movement exclusively, but certain movements usually follow stimulation in a definite portion. The subdivision of function in other areas was studied by these same authors. They endeavored to determine the kind of motion resulting from stimulation. They found that extension of the arm followed stimulation in the upper portion of the arm area, flexion in the lower portion, and that there was a confusion of movement, when the stimulus was applied to the central portion of the area. Extension and flexion in all centres are usually widely represented. Let us carry these facts over to the diagram of the mesal surface of the human brain[1]. The localization of functions on the mesal surface appears in the order of head, arm, trunk and leg from before backwards. The basis for the schematic representation in man is about as follows: some of it is based upon analogy and some on the results of direct stimulation. Below and behind the head and eyes there is an area for the control of the face. The localization of the face area is based primarily upon pathological and clinical evidence and secondly upon analogy with some experimental evidence for its support. The face area can be broken up into a number of other centres. The muscles concerned in speech are represented in this area. The speech centre appears to be a duplication of this representation in a refined form and is usually left out of the discussion of motor centres. It is, however, a motor centre, having fibers which pass into the internal capsule. We know nothing of the subdivisions in it. Has this motor region other than motor functions? Motor reactions follow the stimulation of the cortex outside of what is generally designated as the motor region. Stimulation in the occipital region and in the tip of the temporal lobe gave motor reactions. When the stimulus was applied to the tip of the temporal lobe, reactions in the mouth and nose followed. Movements of the eyes were to be obtained by stimulation of the head area and also of the occipital region of the cortex. These motor reactions were due, according to Ferrier, to the stimulation of the sensory cells in the cortex which in turn reacted so as to bring about movements. Schäfer[2] carried the investigation further.

When he applied the stimulus to the occipital portion of the cortex and produced movements of the head and eyes, he observed that the reaction time was longer, than when the stimulus was applied directly to the head area. By cutting out the proper centre in the head area and then stimulating the occipital portion, reaction was still obtained, which showed that motion in this case was independent of centres in this area. This is but one example of apparent multiple representation of movement in the cortex.

Lecture IV.

Gentlemen: We shall consider this evening the sensory centres. The motor centres form a dividing line in the cortex, behind which

1 Mills. Trans. Cong. Am. Phys. and Surgeons, 1888.
2 Schäfer: Internat. Monatschr. f. Anat. u. Physiol. Leipz., 1888.

lie the sensory centres, and in front is an unoccupied area which is left out of the discussion. The anatomy of the sensory region needs some attention. If we section the white matter in the motor region, degeneration follows both toward the thalamus and toward the cortical cells. The same holds for the sensory region. These fibers are, therefore, arranged to carry impulses both ways, that is, there are both afferent and efferent fibers. Ferrier began the study of the sensory centres. By stimulating the sensory centres he was able to produce motion, but the motion was particularly associated with the peripheral sense organs. Permit me to call attention to a peculiarity of the sensory region. In the lower animals sensation is not so accurately located as motion. The reactions we have to study are crude. Slight loss of sensation can not be shown. The results of the study are often contradictory and no reconciliation seems at times possible. All the results that are accumulated are not of equal value. I shall make use of some of those results which appear most trustworthy. When one set of experiments support a view with good positive evidence, and the opposite view is sustained by equally good positive evidence, there is reason to think that a further extension of the hypothesis will harmonize the views.

Let us take up first the cortical centres for vision. The experiments have been made upon monkeys. In them we have the occipital lobe and the angular gyrus as cortical centres for vision. In man the cuneus is supposed to be connected with vision. The discussion that has taken place has been concerned with the relative values of the occipital lobe and the angular gyrus as centres for vision. The evidence is this: Brown and Schäfer[1] removed the occipital lobe on the left side and the result was a defect of vision in the left halves of the two retinae. This result was persistent, no recovery of vision occurred. They then removed the occipital lobes on both sides and complete blindness resulted. Again the results were permanent. Ferrier criticises this on the ground that they injured the angular gyrus. Brown and Schäfer removed the angular gyrus on one side and no permanent defect of vision followed; even when they removed it upon both sides, the defect was not permanent. The criticism that is made here is that they gave attention simply to permanent defects. The removal of the angular gyrus caused transient blindness in the opposite eye. The animal could see objects afar off but could not see them so well near to, and this was apparently a persistent symptom. Ferrier[2] removed the occipital lobe and found no disturbance of vision on either side. However he left the ventral portion of the lobe intact and this is used as an argument against his results. When he removed the angular gyrus, the opposite eye was affected transciently; but vision returned after a time. When the other gyrus was removed, the other eye was very much affected, and the eye upon the same side slightly so. He did not get a blind monkey from his operations upon either the angular gyrus or the occipital lobes alone. When he removed both the occipital lobes and angular gyri, his monkey became permanently blind. The removal of the occipital lobe from which he got little or no effect, and of the angular gyrus from which he got transcient effects, when each was removed separately, gave permanent and real blindness, when removed together (!). Transient symptoms in lower animals may become permanent in higher animals. Becterew[3] has published an account of his experiments upon dogs and rabbits. With the removal of the occipital lobe, hemiopia occurred . The removal of the angular gyrus produced amblyopia in the opposite eye. We pass now to the clinical evidence in man. The region of the cuneus is usual-

1 Phil. Trans. Roy. Soc. London, 1888.
2 Lancet. June and July, 1890.
3 Neurologische Centralblatt, No. 8, 1890.

ly described as the principal centre of vision. Reported lesions mainly occur in the apex of the cuneus and extend from the occipital lobe into the angular and supra-marginal gyri. Hemiopia follows lesions in the occipital lobe. In the neighborhood of the angular gyrus, lesion produces crossed amblyopia. The same relations hold in man as in monkeys. Some subdivision of the optic centre has been made out. If the lower portion of the occipital lobe in the monkey be stimulated an upward movement of the eyes is produced. Stimulation in the middle portion causes lateral movements and in the uppermost portion downward movements. (Schäfer[1]) This has been interpreted to mean a detailed projection of the retinae upon the cortex. A partial decussation of the optic tracts would account for the location of half of each retina in each occipital lobe. Some clinical evidence for sub-division in man has been presented (Hun. Amer. Jour. Med. Sci. Jan., 1887.), but is at present insufficient.

Among birds a partial decussation of optic tracts has not been observed anatomically. In owls, however, it would seem to occur. Ferrier found that when the occipital lobe was removed, the eye of the opposite side became blind to tests applied. For purposes of experiment the sound eye was first completely bandaged; it was next enucleated, when the blind (?) eye showed enough sensitiveness to enable the bird to catch a mouse in its cage. It would appear that the small sensation here was inhibited by the simple presence of the sound eye. The inference then is that there is a partial decussation in this bird. From our present point of view, hemiopia is dependent upon partial decussation and partial decussation must be supposed to exist, when hemiopia occurs. We now pass to the auditory sense and its location. Here the positions held by the experimenters are at present irreconcilable. Ferrier stimulated the posterior portion of the superior temporal gyrus in monkeys and got a movement of the ear. Excision of this gyrus produced deafness, when the excision was made upon both sides. Brown and Schäfer removed this region and deafness did not follow. In man the clinical evidence favors Ferrier. There are two cases at least where lesion in the posterior portion of the superior temporal gyri caused complete deafness in man. In the auditory form of aphasia this region is undoubted the auditory centre.

Taste and smell are of but little importance in this connection. In this case stimulation indicated the tip of the temporal lobe as their probable centres. If, as we suppose, the discriminative use of an organ determines its representation in the cortex, these centres would then have small cortical representation. The same disagreement exists here among the investigators. Ferrier finds lesions here to produce a loss of taste and smell; Brown and Schäfer find the opposite results. Let us consider now the cutaneous sensibility, and here experimentalists are in accord upon the main issue. Ferrier produced a disturbance of cutaneous sensations by the removal of the hippocampal gyri. Horsley and Schäfer[2] followed up these results and removed the gyrus fornicatus in monkeys, when hemi-anæsthesia of the opposite side of the body followed. Partial removal was tried, but the disturbance of sensibility to pain and tactile stimuli for different segments of the body was not localized. This cortical centre appears to be connected, mainly, but not entirely with the opposite side of the body. The symptoms of allochiria followed lesion on one side. The animals thus operated upon appeared to recover after a time. The degeneration following this lesion in monkeys has been studied very carefully by France.[3] In the internal capsule it could not be definitely located, but in the crura and the pyramidal tracts the location was clear-

1 Proc. Roy. Soc. London, Vol. 43, 1888.
2 Phil. Trans. Roy. Soc. London, 1888.
3 Phil. Trans. Roy. Soc. London, 1889.

ly made out. The important point here is the degeneration of sensory fibers downward, and their presence in the pyramidal tracts in the cord and further the observation that they occupy a definite position in these tracts. France appears to have guarded against any confusion of the lesion described with the lesions following injury to motor centres. Nothing can be said at present of the frontal and ventral portions of the cortex, so we next pass to the localization of lesions in aphasia. Starr[1] gives three periods in the development of our knowledge of aphasia. The first is that of Broca-motor asphasia—the second, that of Wernicke—sensory aphasia—and the third, that of Charcot—a further analysis of sensory aphasia. According to Charcot the idea of an object for the educated man is dependent upon two sensory centres—auditory and visual, and is capable of expression either by the spoken or written word. We have then four principal centres to consider. In visual and auditory aphasia lesions occupy definite areas. For motor aphasia the motor centre is the speech centre. Sensory lesions occur mainly in the region behind the fissure of Sylvius; auditory disturbances being associated with lesions of the superior temporal gyrus, and visual disturbance with those of the angular gyrus. Where the motor centre for writing may be, is not clear. It does not appear, however, to be within the arm area and may possibly hold a relation to this area similar to that which the speech centre holds to the face area. The connection between the sensory and motor centres involved is probably made by association fibers which pass beneath the island of Reil. The sensory form of aphasia is capable of very considerable subdivision, and seems destined to yield results of much psychological importance. I would call attention to the fact that even Charcot's scheme is capable of extension and that aphasia or its intellectual equivalent would be in a deaf mute a lesion in the pathway formed between the centres for cutaneous sensibility, and that for the movement of the fingers in the arm area. That in other words any sensory centre may form the first link and any motor centre the second, and with this may be associated the intellectual life of the individual.

Of the processes occurring in nervous system, none perhaps contribute more to our anatomical information than that of degeneration. Degeneration is, however, a very complicated process. In the higher animals a section of nerve fibers within the central nervous system is not followed by reunion. The nature of the degeneration which follows such a section is dependent on a large number of conditions. To take an example which is related to the question of the representation of the cortex in the thalamus which we have just discussed: If in the rabbit the motor fibers coming from the cortex be sectioned in the crura, the distal portion alone degenerates. If the section of these same fibers be made between the internal capsule and the cortex, not only the distal but the proximal portion with its associated cell degenerate. The reason for this is by no means clear, but may be dependent on the connection of this region with the thalamus. The peripheral sensory nerves furnish an example, where the direction of the nerve impulse and direction of degeneration are dissimilar. In a very young animal separation of a motor nerve at the point where it emerges from the central nervous system is often followed by complete absorption,[2] of both nerve and cell within the nervous axis. If a somewhat greater length of nerve be left attached to the central portion, atrophy only, and not absorption occurs.

Whether the portion within the central axis is absorbed, because, in the first instance, too much has been removed, or because the part removed had a special nutritional value from its position, must be left undecided. But the ultimate disappearance of the residual portion seems

1 Trans. Cong. Am. Phys. and Surg. Vol. I. 1888.
2 Forel: Arch. of Psychiatrie B. XVIII, 1887.

in some way to depend on the struggle for existence among the cell elements in the growing organism. The failure of nerve fibers to unite within the central nervous system might be thought to have some relation to that curious interdependence between growth and specialization by which the one is exclusive of the other. But on the whole it appears as though the conditions of nutrition would offer the best explanation for what occurs.

LECTURE V.

Gentlemen: Permit me to call attention to certain facts which are somewhat aside from the direct line of the previous lectures. We have spoken as if cerebral localization were an absolute fact, and such is practically the case, when we confine our attention to man and the monkeys. If, however, we study cerebral localization in the vertebrate series, we find that it becomes less perfect as we pass downwards. In this matter, comparative anatomy is the starting point. I have here drawn the brains of a dog and of a bony fish. In these widely separated forms, the anatomists can identify the subdivisions of the encephalon, which are homologous. From the physiological side, the question of importance is, whether homologous portions of the nervous system have the same relative function throughout this series. Not many years ago, this question was answered in the affirmative. The experiments of Goltz upon dogs which led him to deny cerebral localization in man were based upon this assumption. We propose this evening to present evidence for the view that homologous portions of the nervous system have not the same relative function in the lower as in the higher vertebrates. The problem may be expressed in anatomical terms; if we attempt to depict the paths along which an impulse must pass from the time it enters until it leaves the central nervous system, we have some such scheme as this: A nerve fiber comes from the periphery, is interrupted by a cell in the spinal ganglion, and enters the cord by the dorsal cornua; it connects itself in some way with a central cell. This cell is in turn connected with a motor cell, from which a fiber passes out of the cord by the ventral root. An impulse thus enters by the sensory and passes out by the motor fiber. This pathway for an impulse occurs from one end of the nervous system to the other, and may be designated as the segmental pathway. On this segmental system, composed of the spinal ganglia, the central cells and those giving rise to the motor fibers, there is superposed a mass of material which is represented by the thalami and cerebral hemispheres. The exact relations of these two structures are not known. For our purpose, we may look upon them as forming a part of a long or central pathway, over which an incoming impulse may, in some cases, pass. It is necessary then to elaborate the diagram, and make it possible for a sensory impulse to pass by means of a central cell to the cortex, where, in all probability, another central cell is interpolated in its course; from there, it passes to the cell giving rise to efferent cortical fibers, and so back to the segmental motor nucleus. This latter is the long or central path, which may always be contrasted with the short or segmental path. The application of such a scheme to the question in hand is this: Where the segmental or short path is highly differentiated, we would expect but little control from the cerebral hemispheres; whereas, where the long or central path is highly differentiated, we would expect the function of the cerebral hemispheres to be important, and the segmental path unimportant.

To begin with a fundamental question: Are both paths always permeable? This has been tested by the stimulation of the cerebral hemispheres in different orders of vertebrates, in which operation a certain portion of the long path was made to conduct the impulse, and this portion was thus shown to be permeable. The inference drawn is that if

permeable through a portion of its length, the path was permeable throughout its entire length. If we apply this test at different levels in the vertebrate series, we find, as a general result, that the long path is always permeable, and, at the same time, we find that the differentiation in the cerebral hemispheres as indicated by the specialized character of the response, decreases from the higher to the lower orders; further, that centres where they can be made out. are less clearly circumscribed and subdivided in lower as compared with higher forms. In the very lowest forms examined, like the bony fish, the reaction to stimulation of the cerebrum is so crude and generalized that it is not distinguishable from reactions obtained through the segmental system. To determine whether the segmental path is permeable, the central path must be destroyed and reactions of the animal then observed. In general, it is found that an interrelation exists between the short and long path, of such a nature that the high development of the one is associated with the low development of the other. In the very highest vertebrates, it appears that in most parts of the cerebral system, the segmental paths are not permeable; but in animals below the dog, they certainly are permeable, and the complexity of reaction of which they are capable increases as we pass down the series. Before entering upon a description of the disturbances following interference with the cortex in mammals, such as the dog, I wish to refer to one immediate consequence of the operation. When a portion of the cortex is removed, a considerable number of the conducting fibers, which remain, undergo a secondary degeneration. In the process of dying, these residual fibres must get rid of their energy, and, in so doing, they cannot fail to influence the portions with which they are connected. It is apparently due to this disturbance that the transient phenomena, which act like inhibitions, arise. Turning now to the special cases, we shall commence with the experiments upon dogs, and before we have finished, I shall hope to have presented evidence for the statement that in the vertebrate series at its lowest limit, sensation and motion, spontaneity and choice are independent of the cerebral hemispheres, but that the dependence of these functions upon the hemispheres increases as we ascend in the series.

This figure[1] represents the brain of a dog with the right hemisphere removed. The animal lived more than a year after the last operation. The senses of sight, hearing, smell and taste were more or less impaired. The animal was a stupid creature, but the disturbances of motion were not to be seen until the two sides of the body were compared, when it was noticeable that motion was impaired upon the side opposite to the lesion. In the second diagram is represented a case where the attempt was made to cut out the frontal portions of both hemispheres. In addition to the intended removal, a secondary degeneration of the left occipital region occurred, which left in the end hardly more than one quarter of the hemispheres intact. The dog lived two and a half months after the last operation, and exhibited that ceaseless activity characteristics of dogs from which the frontal lobes have been removed. Voluntarily, it did not take food; but when food was given it, all the mechanical processes of chewing and swallowing were executed. The emotional sounds—barking, whining, growling, etc.—were appropriately used. At the time, when this animal was described, the operation was the most severe recorded for dogs. Since then Goltz has made a complete removal of both hemispheres[2] and the animal lived 51 days after the operation. This individual, for one reason or another, preserved no special senses. There was, however, no paralysis of any muscles, and the dermal sensations were everywhere present.

1 Goltz Pflüger's Archiv., Bd., 42, 1888.
2 Neurologische Centralblatt, May, 1889.

As in the previous case, it required to be fed, but food placed well back in its mouth was properly chewed and swallowed. It moved about spontaneously, would stand upon its hind legs and walk in a co-ordinated manner. When hungry, it was restless; when satisfied, it slept. It could be awakened by a touch upon any part of its body, and, when so roused, it stretched after the manner of waking animals. From the foregoing, it will be seen that the loss of the hemispheres in the case of a dog is a pretty serious matter, but that a dog is still capable of living after such an operation, and preserves at least his dermal sensibility and considerable control over his muscles. As regards the special senses, we can only say that some remnant of vision may remain in a dog thus operated upon.

Christiani[1] experimented upon rabbits, and showed that when both hemispheres are removed, the rabbit can still see and hear, and retains its dermal sensibility. His experiments are open to the objection that the animals were not kept alive for more than two days. The operations, however, were perfect, and observations began immediately after the operation was performed. A rabbit is less disturbed by the loss of its hemispheres than a dog, and it is particularly noticeable that hearing was retained. Birds also have been worked upon. Schrader[2] has done the best work upon pigeons. There is a wide difference in the intelligence and the relative value of the hemispheres among the various orders of birds. When a pigeon loses its hemispheres, it may at once begin to walk or will fly, when thrown into the air. In some cases they fall asleep, and are aroused only by hunger. Often, when placed upon the floor, such a bird will walk continuously until some obstacle stops it for a moment; when stopped, may fall asleep. The bird sees, and this spontaneity is due to the fact that it can see, for it roosts when it becomes dark. It can not feed itself, when its cerebral hemispheres are entirely removed, but the retention of a small portion will suffice for this purpose. It hears slightly, but will not heed the call of other birds. Taste and smell are difficult to demonstrate, even in normal birds. It will choose between two perches the one that is best suited to its purpose.

Reptiles have been experimented upon very little. We may pass at once, therefore, to frogs. A frog[3] is not deprived of all spontaneity, when the cerebral hemispheres alone are removed, that is, when the thalamus remains intact. The frog can jump and feed itself; it avoids obstacles and can see. It burys itself in the winter and awakes in the spring. In fact, the chief difference between the operated and normal frog is in a certain slowness and sluggishness in initiating any action. This discussion is complicated by the fact that the cerebral hemispheres consist of a basal ganglion, over which is spread a mantle. The mantle is functionally the more important in the higher animals, while it is of very little importance in the lowest vertebrates, and in the frog it may be removed without producing observable symptoms. In the bony fish, the mantle can be shown histologically to be non-nervous in structure, and whatever cerebral functions such an animal may have are associated with the basal ganglia. Cutting off all but the thalamus in a fish, there is no apparent loss of function, but the animal appears a trifle more rash. It can balance, swim, play, feed, distinguish between a worm and a piece of string, and select red wafers from an assortment of various colors thrown upon the water.[4]

We have some experiments upon the shark. If the brain is cut off in front of the thalamus, it can not feed, but retires to one side of the aquarium. The animal can see, but this is of no value to it. The same

1 Zur Physiologie des Gehirnes, 1885.
2 Pflüger's Archiv., Bd.. 44.
3 Schrader, Pflüger's Archiv.. Bd. 41.
4 Steiner. Functionen des Central nerven systems und ihre Phylogenese. Zweite Abtheilung, 1888.

effect was produced when the olfactory lobes alone were cut off. The shark depends upon its sense of smell, and, since cutting of the cerebral hemispheres, deprived it of its most important sense, the effect, as we have seen, was serious. The relation of the dominant sense to the cerebral hemispheres must always be borne in mind in estimating the value of these experiments. For when the relation of this sense to the hemispheres is such that it must be necessarily injured, the effect will be far more serious than in those cases where it escapes injury. Witness the removal of the cerebral hemispheres in the shark, where the dominant sense—the olfactory—is necessarily injured, and the bony fish, in which the dominant sense—visual—is not disturbed.

The plasticity of the nervous system is shown by the following experiment:[1] If the cerebrum of a shark be cut out unsymmetrically, forced movements occur; the animal swims in a circle. If a shark be beheaded, the trunk swims in a straight line. If now an animal be taken and an unsymmetrical operation on the brain be performed, so as to obtain forced movements and the animal thereupon be beheaded, the trunk exhibits forced movements — like those before beheading — and which appear independent of any permanent contracture in the muscles. If, however, the time between the production of forced movement and the final beheading be less than eight or ten hours, forced movements in the trunk alone do not occur. It thus appears that a certain length of time is necessary to educate the spinal cord to perform this motion.

LECTURE VI.

Gentlemen: To-night I wish to give some account of the principal explanations which have been offered for phenomena of cerebral localization, and to indicate some of the points of contact between these phenomena and psychology. The explanations that have been offered have been mainly from physiological point of view, and have often lacked a good anatomical foundation. The phenomena which the various authors try to explain may be summarized as follows: The meaning of the movements that follows stimulation of certain portions of the cortex; the meaning of the loss of movement and sensation which follows removal of portions of the cortex; the significance of the permanent or transient character of those symptoms, and where transient, the interpretation of the gradual return of function. Further the degenerations following removal of portions of the cortex are to be explained. The explanations that have been offered have been strongly influenced by the bias of there authors, and differ from one another mainly in the emphasis which they put on similar facts. For example Schiff — Pflueger. XXX, 1883,—was strongly influenced by two previous conclusions, first, that nervous centres were not artificially excitable, and second, that dorsal columns of the spinal cord were the afferent paths for tactile impulses and lesion of them would cause an ataxic disturbance of locomotion. For him the true centres for sensation and motion were in the basal ganglia. The nerve fibers connecting these centres with the spinal cord formed an arch, the summit of which lay more or less close to the surface of the hemispheres. Stimulation of the cortex excited these sensory or aesthesodic fibers and brought about a reflex motion. Deep removal of the cortex injured the efferent or kinesodic fibers and gave rise to secondary descending degeneration. Return of function after injury was due to the taking up of the lost functions by those portions which remained intact. Schiff therefore emphasized the sensory side in his explanation. He looks upon Munk (Functionen der Grosshirnrinde p. 42) as the man nearest the truth in his explanation. Munk was struck by the fact that even out side of the so-called motor regions, stimulation of the

1 Steiner. op. cit.

cortex gave rise to movements. These extra motor movements were particularly associated with the organs of special sense and were obtained from regions of the cortex which were later found to be the centres for these special senses. Since then each special sense appears to have definite movements associated with it, Munk was led to regard the motor region of the authors as a cortical centre for tactile sensation, in the wide sense of that term, and the motor responses here were to be compared with the motions of the eye upon stimulation of the visual, or of the ear or stimulation of the auditory region. In order to understand his theory of the restitution of function, we may regard his idea of the visual area, where he has worked out his views in detail. In the first place there is a detailed representation of the retina in the cortex. In the centre of this region are the cells which receive the simple sense impressions and around about this central portion are cells which store visual memories. Removal of the former causes absolute blindness, removal of the latter mind blindness or loss of visual memories. When restitution of function takes place it is by the education of the unoccupied cells in the surrounding regions. If all cells capable of this further development are removed, the animal becomes completely and permanently blind. Wundt has criticised the psychological side of this view with all needful severity and we shall see that the experiments of Goltz satisfactorily do away with any such theory of restitution. Goltz[1] came to the question fresh from the study of the spinal cord and apparently convinced of the general truth of Flourens' view, that there was no specialization of function in the hemispheres. In his general view of localization, Goltz is as far as any one from that which is demanded by the clinical medicine of to-day. At the same time he has contributed a large amount of experimental material which forms one of the most valuable chapters on this subject. In the first place he distinguished sharply between permanent and transient symptoms; the latter were brought about by inhibitions due to the secondary degenerations and other disturbances immediately following the operations. For him what is lost is permanently lost, so that restitution of function is never quite complete; but since each hemisphere is connected with both halves of the body, there may be an apparent return of function due to this fact. When both hemispheres are removed, he is forced to the segmental centres, as explained in the last lecture. In this controversy and especially against Goltz, it has been from time to time urged that the removal of a centre or a region was not complete and hence the functions were not abolished. Here and there some evidence appears that the physiological value of a small portion of the cortex may be out of all proportion to its actual size; but we can say nothing more on this point until it has been subjected to a direct experimental test. Hitzig[2] from the experimental side and Nothnagel[3] have emphasized the idea that disturbances of the muscle sense are the cause of the motor disturbance observed. To this view Bastian[4] has added the suggestion that the motor regions, besides being centres for the muscle sense, were also centres for obscure sensations—kinæsthesis—which informed us of the state of contraction of all the muscles of the body, and thus profoundly influenced the contraction of any given muscle. Bain and Wundt have added the sense of effort as a function of the efferent nerves; but this needs hardly to be taken into account, since Prof. James' criticism of their view. I mention these points to show how much vested interest there is in the various theories of the muscle sense, rather than to emphasize their import-

1 Verrichtungen des Grosshirns, 1881.
2 Untersuchungen 1874.
3 Virchow's Archiv. 1873.
4 Brain as an organ of mind, 1880.

ance for our present consideration. Brown-Sequard[1] has emphasized the idea of inhibition. For him both sides of the body are represented in each hemisphere of the brain and the usual symptoms which are taken to indicate motor and sensory centres in the cortex, are but the results of the inhibitory effect of lesions acting upon centres elsewhere situated. He attempts to support his view by clinical evidence to show that lesions almost anywhere in the cerebrum may produce similar symptoms. The method of proof is open to grave objections, and Brown-Sequard has few if any disciples. At the same time it is not impossible that some of the instances showing defect on the same side as the lesion may be true and explicable on the assumption of anomilies in the structure of the nervous system.

Without further entering upon the history of these views, I will at once proceed to give the explanation so far as it seems well supported by anatomy. Goltz has shown that the disturbances in locomotion and other movements cannot be properly explained by referring them to defects in sensation. For our general explanation we come back to the idea developed in the last lecture of a short segmental, or a long central path. To use the term sensory with regard to the afferent and motor with regard to the efferent portion of the central path is to a certain extent misleading. The terms are, however, in common use, and, if we can escape attaching too much value to them, are satisfactory. The course of the incoming impulse over the sensory path is yet to be made out. On leaving the cortex by the motor path its course seems comparatively clear. It is quite impossible to say whether in stimulating the cortex we stimulate the sensory cortical elements, and thus influence the motor ones or stimulate the motor directly. Any general scheme must also explain the restitution of function and this term may stand for a number of different events, especially in the higher animals. As Horsley[2] has shown the restitution of function in the case of hemorrhage into the internal capsule may be well associated with the resorption of the clot. Again the muscles of the phonation and mastication have in man a bilateral cortical representation so that as a rule these muscles on both sides of the body are represented in each hemisphere. Here of course the explanation of restitution is comparatively simple and it is this explanation which is also used in case of dogs with a single hemisphere. In fact bilateral representation appears to increase as we pass down the vertebrate series. There seems to be no evidence to show that when the arm is paralyzed through cortical lesion that there is here, in man at least, a restitution of function. But there is still another sort of restitution which differs mainly perhaps from the fact that it is more complex. I refer to such cases as those in which articulate speech is reacquired after destruction of the motor speech centre upon one side. About such an instance cluster a number of interesting problems. We know that as a rule for speech, both motor and sensory, the left hemisphere is the more important. Why this is the case is nevertheless not clear. That the important sensory and motor centres, which are in practice interrelated, should come to be in the same hemisphere, seems a natural result of the better anatomical connections between these centres on the same side of the brain; but whether in the determination of this side the motor or sensory element takes the lead, we cannot say. At the same time it would appear that both hemispheres of the brain share in the education even in those cases where the exercise seems to be limited to one side of the body. If this is so, then, the reacquisition of language by an adult, after loss of the motor centre for speech on one side, would perhaps be dependent upon this double education of the brain and the possibility of establishing connections between the sensory and mo-

1 Lancet, 1876, 1877.
2 Lancet, 1884.

tor centres in the hemisphere which had not been explicitly exercised. This necessarily brings to our consideration the manner in which we are to think of these associations as taking place. The long period of helplessness in the highest mammalia, the evident effect of training and exercise in the earliest years of life, would seem at first to point to the establishment of new morphological relations as the result of functional activity. A closer examination brings out a good deal of evidence against this view. If functional superiority has a morphological basis, then the left side of the brain should in most instances be the more largely developed. In the average individual the difference in weight between the two hemispheres lies within the errors of observation. We must consider that, if a morphological change is brought about, it is practically the same in both hemispheres. Further, in Laura Bridgman, the portions of the hemispheres connected with her defective senses, though in some instances slightly abnormal, were by no means lacking nor histologically degenerate. So far as we know she had neither visual or sensory memories. Considering growth and function as closely related, then the growth in these portions of her hemispheres was certainly remarkable. I am inclined to the view that the morphological characteristics of a brain are very early fixed and that education has to do mainly with functional developments hence, in the case we have been considering, the reacquisition of speech would depend on association paths which had already existed. The sensory regions of the cortex have a peculiar interest and value, for it seems on the one hand that the ideational of processes are most closely linked with the sensory regions and that on the other hand a single sensory region may serve as the basis for an intellectual life: witness the mental development of the blind deaf-mutes. It may be safely said that in acuteness, man is surpassed by some animal in the case of every sense. Man is peculiar in the high development of several senses and in the ability for cross-reference between them, so that, although each principal sense at least would seem to be sufficient for a basis of an intellectual existence, and thus each sensory region might be considered a little brain, yet fullness of intellectual development would appear to associate itself with a high simultaneous development. On this point the manifold symptoms of aphasia are most instructive.

NOTES ON MODELS OF THE BRAIN.

HENRY H. DONALDSON, Clark University.

1. The use of models of the brain as one means of instruction requires no apology. In view of the interest which at present attaches to such models, I have made a list of the principal ones with some annotations. At my suggestion, Mr. T. L. Bolton has prepared a translation of the description of the large brain-model manufactured by Auzoux. This model appears to be, on the whole, the most instructive one, and, as the original description was in French, the anatomical terms of which are, as a rule, unfamiliar to our students, it was thought that such a translation of the description would make it more generally useful.

LIST OF MODELS.

1. Aeby's wire model of brain and cord: (Phantom des Faserverlaufes im menschlichen Gehirn und Rückenmark von Prof. Dr. Chs. Aeby). Made by F. R. Büchi, Meckaniker, Berne, Switzerland. Price, 500 francs. Material, wire and cork. Shows the path of the fibres according to Wernicke and is enlarged about six diameters. Useful from the fact that, though giving the relations in three dimensions, it is transparent.
2. Auzoux (Mme. Ve Auzoux, 56 Rue de Vanguard, Paris). The synthetic preparation of the brain (Cerveau de Texture de tres-grande

dimension. Prix, 300 Fr. Enlargement about two diameters.) is the one to which the following description applies. It is a valuable preparation from the fact that is shows the inter-relations of the deep portions. Auzoux makes a number of other preparations of both brain and cord, which are less elaborate and for which the reader is referred to his catalog. Material: Papier maché.

3. Exner. In the physiological institute of the University of Vienna, there is made a plaster cast of cerebrum—life size—on which are marked the cortical centres as determined by Exner,-vide. "Localization der Functionen in der Grosshirnrinde des Menschen." Price, 10 Marks.

4. Fick. Phantom des Menschenhirns. Ludwig Fick—natural size—paper. Price, 2 M. This is an attempt to show the relations by layers of paper cut in the proper shape and colored. It is a very helpful device.

5. Tramond. Maison Vasseur, 9 Rue de l'école de medicine, Paris. Among the very numerous models made by this firm, a number relate to the central nervous system. "Anatomie du bulbe rachidien grossi par 13 coupes, dáprès Mathias Duval." Price, 80 francs. (Plaster of Paris.) The transverse sections admit of identifying the appearance of a section with its level in the medulla. The preparation is rather rough, especially in the painting of the sections.

6. Ziegler. Dr. Adolph Ziegler in Freiburg. Baden, Germany. This maker works in wax, and his preparations are both accurate and artistic. The gyri of human brain — Ecker — shown in two pieces — life size. 30 Marks. The development of the gyri in the human brain—Ecker—shown in 14 pieces—life size. 54 Marks. Form of brain in the vertebrate series: 8 pieces, somewhat enlarged. Price, 136 marks. These three sets of models are very useful for the study of the development of the gyri in man and for a general idea of the comparative development of the brain in vertebrates.

The above list represents but a few of the available models, but I believe it contains those which will be found most serviceable for ordinary purposes. A model can be constructed by enlarging cross sections of a part and drawing them on large panes of glass. These panes arranged in series in a frame thus give a fair idea of the part, being at the same time transparent. We have thus constructed a model of the spinal cord in man. An accurate opaque model can be made from plates of wax of proper thickness, on which the enlarged sections of an object are outlined, and then the portion so outlined is cut out. These plates of wax, when piled together, will then represent the object enlarged so many diameters.

To the following translation I wish to preface a few words. The model described is based on the views of Dr. Luys. In some instances these can be shown to be wrong, and, in others, open to serious objections. In the main they agree with the anatomy of the day. It is not our purpose to edit the description, any further than is needful for the present case. All that appeared superfluous in the original description has been omitted in the translation. After the erroneous or doubtful designations, we have for the most part contented ourselves with putting [Luys] in brackets, to designate that the view presented by that author was not generally accepted.

We have introduced some synonyms. In the cases which are most complete, the English, Latin, Terms of Wilder, French and German are given in the order named, but we have been guided here more by what we thought would be of assistance, than by the idea of formal completeness.

BRAIN MODEL ON A LARGE SCALE.

By Dr. Auzoux.

Translated by T. L. Bolton.

Explanatory Remarks.—An ordinal number preceded by the sign ☞, indicates that the piece upon which it is placed may be detached; the small numbers or letters of the alphabet indicate the details.

In this new edition of the human brain, the course of the nerve-fibres can be traced through all the parts of the encephalic mass. This preparation was constructed from dissections made on normal brains hardened in chromic acid according to the directions of Dr. Luys.[1] . . . It enables one to see many details in the cerebrum, in the cerebellum, in the pons, in the medulla oblongata, and in the cephalic part of the spinal cord.

☞ No. 1.

Left half of the callosum — great commissure, corpus callosum, callosum, corps calleux, der Balken.

The callosum, from which the cerebral mass is almost completely separated, represents a kind of box which forms the walls of the lateral ventricle.

The three kinds of nerve-fibres which enter into the composition of the cerebrum are represented in arbitrary colors of different shades. They are designated by the names: Afferent or sensory fibres, efferent or motor fibres, and commissural fibres; those which connect the two hemispheres.

1. Dorsal surface of the callosum.

2. Cephalic extremity of the collosum — the knee, genu corporis callosi, genu, genou die Balkenknie.

3. Reflected portion of the callosum—*E*—, rostrum corporis callosi, rostrum, bec, der Schnabel.

4. Caudal extremity of the callosum corresponding to the splenium—*E*—, splenium corporis callosi, splenium, bourrelet, der Balkenwulst.

5, 5. Longitudinal tracts — nerves of Lancisi — mesal longitudinal striæ, striæ longitudinales mediales, *W*—, nerfs de Lancisi, *G*—.

6, 6, 6. Transverse tracts — transverse striæ, *L*—, *W*—, tractus transvesaux, *G*—.

7 *a*, *a*, *a*. Afferent or sensory fibres [fibres convergentes supérieu res (Luys)].

8 *b*, *b*, *b*. Commissural fibres [fibres commissurantes (Luys)].

9 *c*, *c*, *c*. Efferent or motor fibres [cortico-striées (Luys)].

10. Portions of the corona of Reil — fibrous cone, corona radiata corona, couronne de Reil, der Stabkranz.

11. Ventral surface of the callosum.

12. Sphenoidal boundary of the lateral ventricle (étui de l'hippocampe).

13. Posterior horns of the lateral ventricle — posterior horn, cornu posterius, postcornu, cavité digitale, das Hinterhorn.

14. Unciform eminence, calcar avis, calcar, ergot de Morand, der Vogelsporn.

ci 15. Calcarine fissure, fissura calcarina, calcarine fissure, repli de la rconvolution de l'ergot, *G*—.

☞ No. 2.

Left half of the fornix,—*E*—, fornix, fornix, voûte à trois piliers, die Gewoelbe.

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[1] Recherches sur le système nerveux cérébro-spinal,—sa structure, ses fonctions et ses maladies,—accompagné d'un atlas, par J. J. Luys, Paris, 1865.

Upon this portion may be noticed the anterior pillar of the fornix, its continuation with the hippocampus major, the insertion of the afferent fibres of this region in the gyri of the hippocampus, the commissural fibres that arise in the cortical cells and unite to form the lyra, and how as fibres of the lyra, on reaching the anterior pillar they decussate to form the anterior commissure (Luys).

1. Anterior pillar, columna fornicis anterior, fornicolumna, pilier antérieur, vorderer Gewoelbeschenkel.
2. Posterior pillar, columna fornicis posterior, *W*—, pilier postérieur, hinterer Gewoelbeschenkel.
3. A portion of the lyra—*E*—, lyra, lyra, lyre, die Leier.
4. *E*—, hippocampus major, hippocampus, hippocampe ou corne d'Ammon; Ammonshorn.
5. *E*—, fimbria, fimbria, bandelette de l'hippocampe ou corps bordant, der Saum.
6. Hippocampal gyrus—*E*—, gyrus hippocampus, *W*—, circonvolution de l'hippocampe, *G*—.
7. *E*—, uncus, uncus, crochet, der Haken.
8. The free border of this gyrus which is continued upon the callosum under the name of the nerve of Lancisi, is called dentate convolution,—fascia dentata, fasciola, corps godronné, gezähnte Leiste.
9. The cortical or gray matter—ectocinerea—composed of two layers of cells.
10. The white or fibrous matter—alba (*W*).
11. A transverse section of the hippocampus showing the windings of the medullary fibres and their insertion in the cortical cells.
12. Termination of the anterior commissure—commissura anterior, præcommissura, commissure antérieure, vordere Commissur—in the cephalic portion of the sphenoidal lobe.
13. Posterior extremity of the callosum—the splenium of the authors.

☞ No. 3.

Superior portion of the left optic thalamus—thalamus opticus, thalamus, couche optique, der Sehhügel.

This section shows a portion of the three posterior centres of the thalamus, the fibres of the optic nerve passing from the geniculate bodies to the mesal centre.

1. The thalamus showing:
2. The mesal centre receiving the optic fibres—inner nucleus, nucleus cinereus internus, *W*—, centre moyen (Luys), innerer Kern.
3. The median centre receiving the fibres of the dorsal column of the spinal cord [centre médian (Luys).] (Considered by other authors as a portion of the lateral nucleus.)
4. Posterior centre—centre postérieur—receiving the auditory fibres.
5. Plexiform disposition of the fibres that pass from the thalamus to enter into the formation of the corona.
6. The central tubular gray matter—*L*—, entocinerea, substance grise centrale, centrales Höhlengrau—which covers over the thalamus.
7. Optic tract, tractus opticus, tractus opticus, bandelette optique, der Sehstreif.
8. External root passing to the external geniculate body—corpus geniculatum externum, geniculatum externum, corps genouillé externe, äusserer Kniehöcker.
9. Internal root passing to the internal geniculate body—corpus geniculatum internum, geniculatum internum, corps genouillé interne, innerer Kniehöcker.
10. External geniculate body.
11. Internal geniculate body.

12. Fibres passing from the geniculate bodies to the corpora quadrigemina—quadrigeminal bodies, corpora or tubercula quadrigemina, corpora quadrigemina, tubercules quadrijumeaux, die Vierhügel.

13. Optic fibres passing from the geniculate bodies to the mesal centre of the thalamus; the fibres are laid bare by the removal of the central tubular gray matter.

☞ No. 4.

Upper portion of the intra-ventricular corpus striatum — caudate nucleus, nucleus caudatus, striatum caudatum, corps strié intra-ventriculaire, geschwänzter Kern.

☞ No. 5.

Annular protuberance or Pons Varolii—tuber annulare, pons, pont de Varole, die Brücke—

Showing the decussation of the fibres of the middle peduncle of the cerebellum with the longitudinal fibres of the ventral peduncles of the cerebrum — a decussation which gives to this portion the appearance of a mat.

1. Fibres of the middle peduncle of the cerebellum—*E*—,crus ad portem, medipedunculus, pédoncle cérébelleux moyen, der Brückenschenkel.
2. Decussation of these fibres in the median line.
3. The decussation of these same fibres with the fasciculus of the ventral pyramids — pyramides anteriores, pyramides, pyramides antérieures, die Pyramiden.
4. The fibres of the pyramids.
5. Trigeminal nerve or fifth pair composed of two fasciculi—nervus trigeminus, *W*—, nerf trijumeau, die dreigetheilte Nerv.
6. Motor fasciculus of the same nerve.
7. Sensory fasciculus dividing into two branches.
8. A branch uniting in the formation of the fillet — lemniscus, lemniscus, fillet de Reil, die Schleife.
9. A branch passing towards the central gray matter of the axis.
10. Abducent nerve or sixth pair — nervus abducens, *W*—, nerf moteur oculaire externe, aüsserer Augenmuskelnerv.

☞ No. 6.

Right half of the cerebellum — hind-brain, cerebellum, cerebellum, cervelet, das Kleinhirn.

1. Exterior surface of the cerebellum.
2. Median lobe or vermis of the cerebellum—the worm or vermiform process, lobus cerebelli medius, vermis, vermis du cervelet, der Worm.
3. Lateral lobe of the cerebellum.
4. Lobules or subdivisions of the cerebellar hemispheres.
5. Superior peduncle of the cerebellum—crus ad cerebrum, præpedunculus, pédoncle cerebelleux supérieur, der Bindearm.
6. Middle peduncle of the cerebellum.
7. Inferior peduncle of the cerebellum — crus ad medullam, postpedunculus, pédoncle cérébélleux inférieur, der Medullarschenkel.
8. Valve of Vieussens, velum medulare anterius, valvula, valvule de Vieussens, vorderes Marksegel.
9. Disposition of the fibres of the cerebellum — *E*—, arbor vitæ, arbor, arbre de vie, der Lebensbaum.
10. The folds of the cortical matter forming the folia.
11. Section through the lateral lobe.
12. Rhomboidal or dentate body, corpus dentatum, dentatum, corps dentalé, *G*—.
13. The fibrous portion.
14. Cellular portion.
15. Divergence of the white fibres.

16. Nuclei of the gray matter lying among the foregoing fibres. [Luys]

17. Section of the cortical gray matter.

18. *E*—, uvula, cerebelli uvula, luette, die Zapfe.

19. Valve of Tarini, or posterior medullary velum, velum medulare posterius, metatela, valvule de Tarin, hintere Hirnklappe.

20. Lobule of the pneumo-gastric—*E*—, lobulus appendicularis, flocculus, lobules du cervelet, die Flocke.

☞ No. 7.

Lemniscus or ribbon of Reil and the left half of the corpora quadrigemina.

The fibres which compose the lemniscus have three distinct origins, and after having decussated on the median line, come to lie dorsad of the Iter, and are distributed to the posterior and median centres of the thalamus. The dorsal fibres of this tract decussate and form the posterior commissure [Luys].

1. Anterior tubercle, corpus quadrigeminum anterius, prægeminum, tubercule supérieur, obere Zweihügel.
2. Posterior tubercles, corpus quadrigeminum posterius, postgeminum, tubercule inférieur, untere Zweihügel.
3. Central tubular gray matter covering the fourth ventricle.
4. Fasciculus formed by the trigeminal nerve (Luys).
5. Fasciculus formed by the auditory nerve—nervus auditorius, *W.*—, nerf acoustique, der Hörnerv.
6. Fasciculus formed by the spinal cord—medulla spinalis, myelon, moelle épinière, das Rückemmark.
7. Decussating fibres.
8. Portion of the aquæduct Sylvii, aquæductus, l'aqueduc de Sylvius, die Wasserleitung.
9. Portion of the valve of the Vieussens.
10. Trochlear nerve or fourth pair—nervus trochlearis, *W.*—, nerf pathétique, der Rollmuskelnerv.

☞ No. 8.

Left ventral column of the spinal cord, ventral peduncles of the cerebrum (the ventral pyramids of the authors.)

This portion is designed to show how all the efferent fibres (cortico-striées-Luys) traverse the extra-ventricular portion of the corpus striatum, thus forming the three arches 3, 4, 5; and how, after having formed the three arches, the cortical striæ form the three cones which constitute the ventral peduncles of the cerebrum, that is, the motor tracts.

1. Extra-ventricular nucleus of the corpus striatum cut vertically—lenticular nucleus, necleus lenticularis, lenticula, noyau extra-ventriculaire du corps strié, der Linsenkern.
2. The termination of the fibres of the superior peduncle of the cerebellum in the three arches of the corpus striatum. (Luys.)
3. Internal arch.
4. Middle arch.
5. External arch.
6. Locus niger of Vicq-d'Azyr. (Substantia nigra of the authors.)
7. Tuber cinereum, torus, corps cendré, *G.*—
8. Infundibulum, infundibulum, infundibulum, tige pituitaire, der Trichter.
9. Corps cendré=tuber cinereum.
10. Optic tract.
11. Optic commissure—chiasma, chiasma nervorum opticorum, chiasma, chiasma des nerfs optiques, die Sehnervenkreuzung.
12. (Omitted.)
13. Olfactory tract, tractus olfactorius, *W*—, nerf olfactif, *G.*—

14. Mesal root. (Luys.)
15. Median root. (Luys.)
16. Median root of the right side passing to the olfactory ganglion of the left side. (Luys.)
17. The fibres that bind the olfactory ganglion to the central gray matter. (Luys.)
18. Lateral root, stria externa, *W*—, racine externe, laterale Wurzel.
19. Olfactory ganglion. [Luys]
20. Portion of the tænia semi-circularis or stria cornea, tænia, lame cornée, der Grenzstreif.
21. Anterior commissure—commissura anterior, præcommissura, commissure antérieur, vordere commissur—exposed by the removal of a portion of the corpus striatum.
22. Oculo-motor nerve, or third pair, nervus oculomotorius, *W*—, nerf moteur oculaire commun, der Augenmuskelnerv.
23. Trochlear nerve or fourth pair.
24. Anterior peduncles of the cerebrum formed by the three cones.
25. (Omitted.)
26. (Omitted.)
27. (Omitted.)
28. The fibres of the middle peduncles of the cerebellum intercrossing with the fibres of the cerebral peduncles.
29. The fibres forming the sensory root of the trigeminal nerve, or the fifth pair.
30. The fibres forming the motor root of the fifth pair.
31. The fibres forming the root of the abducent nerve or sixth pair.
32. After-brain—medulla oblongata, oblongata, bulbe, das verlängerte Mark—upon which may be distinguished:
33. Anterior pyramids, corpus seu eminentia pyramidalis, pyramis ventralis, pyramide antérieure, *G*—;
34. Left olivary body—oliva, oliva, olive, die Olive—cut to show the arrangement of the arcuate fibres—fibræ arciformes, *W*—, fibres arciformes, die Bogenfasern.
35. Arcuate fibres derived from the inferior peduncles (Luys) of the cerebullum decussating in the middle line.
36, The hypoglossal nerve or twelfth pair — nervus hypoglossus, *W*—, nerf grand hypoglosse, der Zungenfleischnerv.
37. Ventral column of the spinal cord.

☞ No. 9.

Dorsal peduncle of the cerebrum—tegmentum, tegmentum, étage supérieur du pedoncle, das Haubenfeld.

This portion is designed to show how the nerves which transmit to the cerebrum the sensations from all parts of the body are focused in the four centres of the thalamus, and how the fibres of the thalamus, changing their direction, pass below the tænia semi-circularis to unite in forming the corona.

1. Portion of the corpus striatum—nucleus caudatus.
2. Portion of the corpus striatum—nucleus lenticularis.
3. The thalamus cut transversely.
4. Cephalic or olfactory centre—tuberculum anterius, *W*—, centre antérieur, die vorderer Kern—of the thalamus.
5. Mesal or optic centre.
6. Median or inferior centre.
7, Posterior or acoustic centre — tuberculum posterius, pulvinar, centre, postérieur, der Polster.
8. Posterior commissure, commissura posterior, postcommissura, commissure postérieure, hinterer Commissur.
9. Gray or soft commissure of the thalamus—middle commissure, commissura mollis, medicommissura, commissure grise, *G*—.

10. Mammillary bodies, corpora mammillaria, albicans, éminence mammillaire, die Markkügelchen.
11. Foramen of Monro, foramen Monroi, porta, trou de Monro, *G*—.
12. Anterior pillar of the fornix.
13. Anterior fibres of this pillar proceeding to the gray matter of the septum lucidum—hemi-septum, *F*—, die Scheidewand—and to the corpus striatum.
14. Posterior fibres forming the peduncle of the pineal gland—pedunculus cornarii, habena, pedoncle de la glande pinéale, *G*—.
15. The fibres of this peduncle communicating with the anterior centre of the thalamus.
16. Descending fibres proceeding to the corpora mammillaria.
17. Bundle of Vicq-d'Azyr—radix descendens fornicis, *W*—, fascicule de Vicq-d'Azyr *G*—, proceeding from the anterior or olfactory centre to the corpora mammillaria.
18. Tænia semicircularis.
19. Superior olivary body or the corpus of Stilling—tegmental nucleus, nucleus tegmenti (erroneously called *olive supérieure* by Luys), der rothe Kern.
20. The depression in this body designated as the hilum.
21. Left superior peduncle of the cerebellum.
22. The fibres of this peduncle decussating in the middle line.
23. Inferior peduncle of the cerebellum.
24. The fibres of this peduncle intermingling with those of the dorsal column of the spinal cord.
25. The dorsal column—columna posterior, *W*—, cordon postérieur, *G*—, —of the spinal cord.
26. Restiform body,[1]—corpus restiforme, restis, corps restiforme, das strangförmige Körper.
27. Posterior pyramid,[2] clava funiculi gracilis, clava, pyramide postérieure, *G*—.
28. Floor of the fourth ventricle.
29. Calamus scriptorius continuous with the central canal of the spinal cord.
30. Iter.
31. Gray matter of the axis passing across which the ascending fibres of the posterior peduncle of the cerebellum may be seen.
32. Dorsal column of the spinal cord.
33. Entrance of these fibres in the median centre of the thalamus. [Luys.]
34. Plexiform arrangement of the fibres leaving the centres of the thalamus.
35. Their course below the tænia semicircularis.
36. These fibres ascending to form the corona.
37. Corona.
38. The root of the oculo-motor nerve or the third pair.
39. The root of the trochlearis or fourth pair.
40. Origin of the trigeminal nerve or fifth pair.
41. The motor root of the trigeminal nerve.
42. The root of the abducent nerve or sixth pair.
43. The facial nerve or seventh pair,—nervus facialis, *W*—, nerf facial, der Gesichtsnerv.
44. Nerve of Wrisberg, nervus intermedius, *W*—, nerf de Wrisberg, *G*—.
45. Acoustic nerve or eighth pair.

1 The number and designation should include the more lateral portions of the dorsal column.
2 The designating number 27 is placed upon the funiculus cuneatus instead of upon the funiculus gracilis, where it should stand.

46. The ganglionic enlargement of this nerve—tuberculum acusticum.
47. Root of this nerve losing itself in the central gray matter of the fourth ventricle (striæ medullares or striæ acusticæ of the authors.)
48. The fasciculus of the acoustic nerve contributing to the formation of the leminiscus.
49. The glosso-pharyngeal nerve or ninth pair—nervus glosso-pharyngeus, *W*—, nerf glosso-pharyngien, der Zungenschlundkopfnerv.
50. The pneumogastric nerve or tenth pair—vagus nerve, nervus vagus, nerf pneumogastric, der Herumschweifendenerv.
51. The spinal accessory nerf or eleventh pair—nervus accessorius, *W*—, nerf spinal *G*—.
52. The tubercle of Rolando—tuberculum cinereum Rolandi, *W*—, substance gelatineuse de Rolando, *G*—.
53. The ventral horn of the gray matter of the spinal cord.
54. The dorsal horn of the gray matter of the spinal cord.
55. The ventral or motor root of the spinal accessory nerve.
56. The dorsal or sensory root of the spinal accessory nerve.
57. The root of the hypoglossal nerve or twelfth pair.
58. First pair of cervical nerves.
59. The ventral or motor root of this nerve.
60. The dorsal or sensory root of this nerve.
61. Accessory nerve [Vaso-motor root of the great sympathetic nerve. Luys].
62. Spinal ganglion.

☞ No. 10.

Part of the cortical gray matter of the right hemisphere, on which the gyri and sulci are seen.
1. Fissure of Sylvius.
2. Fissure of Rolando—central fissure, fissura centralis, central fissure, scissure de Rolando, *G*—.
3. The gyri of the island of Reil—insula.
4. The external cortical layer composed of small cells.
5. Internal cortical layer composed of large cells.

☞ No. 11.

Right cerebral hemisphere.

This portion is designed to show in their totality all the parts of the cerebrum and to give an idea of their functions.

1, 2, 3. First, second and third frontal gyri.
4. The fissure of Rolando.
5, 5, 5, 5. Superior or parietal gyri.
6, 6, 6. Posterior or occipital gyri.
7, 7. The fissure of Sylvius.
8. Temporo-sphenoidal lobe.
9. The gyri of the island.
10, 10, 10. The gyri of the mesal surface of the hemisphere.
11. Gyrus fornicatus.
12. Hippocampal gyrus.
13. Fascia dentata.
14. The uncus.
15. The callosum.
16. The splenium of the callosum,
17. The genu of the callosum.
18. The rostrum of the callosum.
19. Longitudinal tract—the nerves of Lancisi.
20. The fornix.
21. The anterior pillar.
22. The septum lucidum composed of two laminæ of which one has been in part removed.

23. The lyra.
24. The thalamus on which four centres are seen.
25. The anterior or olfactory centre.
26. The mesal or optic centre.
27. The posterior or acoustic centre.
28. The median or inferior centre.
29. The pineal gland, glandula pinealis, conarium, glande pinéale, die Zirbeldrüse.
30. The peduncles (habenulæ of the authors.) of the pineal gland separated into two fasciculi.
31. Fasciculus going to the anterior centre of the thalamus.
32. Fasciculus going to the anterior pillar of the fornix. (Luys.)
33. Corpus mammillare.
34. Bundle of Vicq-d'Azyr passing from the anterior centre of the thalamus to the corpus mammillare.
35. The third ventricle.
36. The foramen of Monro.
37. The aqueduct of Sylvius.
38. Anterior commissure.
39. The termination of the anterior commissure in the temporo-sphenoidal lobe.
40. The gray or soft commissure.
41. The posterior commissure composed of three distinct fasciculi intercrossing with those from the opposite side.
42. The tuber cinereum.
43. Infundibulum.
44. Anterior portion of the lateral ventricle.
45. Corpus striatum—nucleus caudatus.
46. Corpus striatum—nucleus lenticularis—separated from the preceding to show the insertion of the cortico-striæ.
47, 47. The termination of the superior peduncles of the cerebellum in the arches of the corpus striatum. [Luys.]
48. The temporo-sphenoidal portion of the lateral ventricles.
49. The hippocampus or Ammonshorn.
50. The fimbria.
51. The fascia dentata.
52. The occipital portion of the lateral ventricles, or the digital cavity.
53. The hippocampus minor.
54. The corpora quadrigemina.
55. The corpus prægeminum.
56. The corpus postgeminum.
57. The great transverse fissure (fente de Bichat)—the space included between the splenium of the callosum, the corpora quadrigemina, and the gyri fornicati. Within this space are to be seen:
58. The geniculate bodies;
59. The fasciculi joining these bodies with the corpora quadrigemina;
60. The floor of the fourth ventricle.
61. The calamus scriptorius.
62. (Omitted.)
63. The superior peduncle of the cerebellum.
64. The decussation of this peduncle with that of the opposite side.
65. The tegmental nucleus.
66. The inferior peduncles of the cerebellum.
67. Tubercle of Rolando.
68. The restiform bodies.
69. The column of Goll—funiculus gracilis, *W*—, funicule grêle, die zarte Stränge.
70. The nucleus of the funiculus gracilis—clava.
71. The olfactory bulb — bulbus olfactorius, rhinobulbus, bulbe olfactif, der Riechkolben.

72. The olfactory tract.
73. The mesal root of this tract.
74. The middle or gray root.
75. The external root going to the ganglion.
76. The olfactory ganglion.
77. The mesal or gray root of the left olfactory tract proceeding to the olfactory ganglion of the right side.
78. The fibres binding the olfactory ganglion to the central gray matter.
79. Tænia semicircularis.
80. The optic nerve.
81. The chiasma.
82. The fasciculus of gray fibres connecting the chiasma of the optic nerves with the tuber cinereum. (Erased in original.)
83. The tract of the optic nerve dividing into two fasciculi.
84. The fasciculus passing to the internal geniculate body.
85. The fasciculus passing to the external geniculate body.
86. Perforated space—anterior perforated space, locus perforatus anticus, præcribrum, quadrilatère perforé, *G*—, —limited posteriorly by the optic fibres, laterally by the fascia dentata, and anteriorly by the roots of the olfactory nerves.
87, 87, 87. The anterior cerebral peduncles, on which are seen the three layers coming from the lenticular nucleus.
88. The root of the common oculo-motor nerve or third pair.
89. The trochlear nerve or fourth pair.
90. The sensory root of the trigeminal nerve or the fifth pair.
91. The motor root of the same nerve.
92. The root of the abducent nerve or the sixth pair.
93. The root of the facial nerve or the seventh pair.
94. Acoustic nerve or eighth pair.
95. Tuberculum acusticum.
96. Lemniscus formed of three roots.
97. Fasciculus of the acoustic nerve [Luys.]
98. Fasciculus coming from the trigeminal nerve (Luys).
99. Fasciculus coming from the spinal cord.
100. Decussation of the lemniscus forming the superior wall of the iter.
101. Part of the velum medullare anterius.
102. The nerve of Wrisberg.
103. The glossopharyngeal nerve or the ninth pair.
104. The pneumogastric nerve or the tenth pair.
105. The spinal accessory nerve or the eleventh pair.
106. The ventral or motor root of the spinal accessory nerve.
107. The dorsal or sensory root of the same nerve. [Luys].
108. The root of the hypoglossal nerve or the twelfth pair.
109. The first pair of cervical nerves.
110. The ventral or motor root of the same pair.
111. The dorsal or sensory root of the same nerve.
112. Spinal ganglion of this same nerve.
113. Vaso-motor fibres of sympathetic nerve. [Luys].
114. Second pair of cervical nerves showing the same character and details as the first pair.
115. Spinal cord.
116. The ventral longitudinal fissure.
117. The dorsal longitudinal fissure.
118. The ventral, anterior or motor column.
119. The dorsal, posterior or sensory column.
120. Lateral column.

121. Gelatinous substance of Rolando in the centre of which is seen the central canal of the spinal cord.

122. The central canal.

123. The ventral horns of the spinal cord in connection with the efferent or motor fibres.

124. The dorsal horns in connection with the afferent or sensory fibres.

125. The central gray matter of the spinal cord.

126. Claustrum, claustrum, avant-mur, vormauer — lying between the lenticulate nucleus and the gyri of the island.

127. Antero-posterior fasciculus composed of afferent fibres, *a*, *a*, *a*, and of efferent fibres, *c*, *c*, *c*, which form the connection between the anterior gyri of the cerebral lobes and the most distant part of the corpus striatum and the thalamus, and which form a kind of enclosure in which the claustrum lies (connections those of Luys).

128. Inter-cortical commissural fibres — association fibres of Meynert.

129. Ventral column of the spinal cord separating from that of the opposite side to allow the lateral columns to decussate.

130. The decussation of the pyramids.

131. The decussation of the dorsal columns.

132. The ventral pyramids.

133. Substantia nigra lying between the two fasciculi which form the anterior pyramids.

134. The ventral layer of the cerebral peduncle — crusta, crusta, *F*—, Hirnschenkelfuss.

135. The superior layer of the cerebral peduncle formed by the dorsal and ventral columns of the spinal cord.

A LABORATORY COURSE IN PHYSIOLOGICAL PSYCHOLOGY.

By Edmund C. Sanford, Ph. D.

After Prof. Ladd's careful statement of the psycho-physiological facts and Prof. James's brilliant exposition of their psychological and even metaphysical import, it is no longer necessary to argue the importance of the subject matter of this branch of the new psychology. No one that has once seen the new is going to be satisfied any longer with the old. But the appropriation of new facts alone is not sufficient to elevate psychology to its true place in the circle of sciences. As long as psychologists live upon the crumbs that fall from the tables of neurology and physiology they will live in dependence. They must investigate for themselves,—no less rigorously and no less broad-mindedly than others, but from their own standpoint, and must view what they find in its psychological perspective. This means that a prominent place must be given to psychological laboratories for research; and the friends of psychology already congratulate themselves on the beginning of several of great promise in this country.

Beyond this, however, lies another thing of cardinal importance, namely, the adoption of a right pedagogical method. The student of psychology must have its facts and principles brought home to him in a way not inferior to the best in other sciences, if psychology is to have the infusion of new vigor that they have had, and afford the healthy and virile training that they afford. He must see for himself the phenomena about which he psychologizes, he must perform the experiments, he must have the inside view. The new psychology has been said to do away with introspection, but that is a mistake. It retains in-

trospection and refines and gives it precision by making it operate under experimental conditions; and it is just these inner aspects that are particularly hard for the student to frame for himself from bare descriptions. He must himself serve as subject of the experiment before he can really understand it. To say, as has recently been said, that a few models of the brain and a color-mixer are about all the apparatus needed for a course in physiological psychology savors of the scholasticism from which we hope to have escaped. Notwithstanding its better material, such a method must lead to the same text-book work and the same artificial general conceptions as of old. For those especially that are to work in any of the fields of applied psychology, in pedagogy, or criminology, or even theology, the intimate laboratory knowledge (and its parallels in anthropological and comparative psychology) is essential to an effective grasp of their subjects. The need of such an apprenticeship for later work in the research laboratory is of course obvious. That such a course is even now desired by open eyed teachers is shown by the inquiries made for it of those known to be engaged in experimental psychological work.

Just what experiments such a course should contain is itself as yet a matter of experiment; but that it should, if it aims at any thing like proportion, introduce the student to all the chief methods of research and cause him to observe for himself all the more important phenomena seems reasonable. Such a course has been in mind in the collection of the experiments which is begun below, and which is to be continued in successive numbers of the JOURNAL till completed. That the list is complete or the selection always the best the author is very far from maintaining — to mention a large omission only, no experiments on hypnotism are now proposed, because they seem unfitted to beginners in the field. And in any event the ideal laboratory course can only be reached after repeated adaptation and long trial in actual use. This course had its origin in a series of notes which it was found necessary to make for the use of a group of students taking my practice course during the past year. The experiments have been performed in the laboratory here, and all, except those added in this revision, by the students themselves. The demonstrational character of the work has been kept in mind, and the experiments chosen are generally rather qualitative than quantitative, even where for convenience they have been given a quantitative form. In selecting apparatus the simplest that promised the desired result has generally been chosen; and while this makes the course by no means representative of the facilities of this laboratory, much less of the possibilities of psychological experimentation, it may perhaps make it useful to those teachers—unfortunately too many—whose equipment must be brought within the compass of a scanty appropriation. A large part of the absolutely essential apparatus could be made by the teacher himself, and almost all, I doubt not, with the assistance of common mechanics. The notes on apparatus and references to literature that are inserted from time to time will open the way to more elaborate experiments and apparatus for those that desire them.

I.—THE DERMAL SENSES.

SENSATIONS OF CONTACT.

Apparatus. The experiments on the Sense of Locality require no special apparatus. Those on Discriminative Sensibility can be made with ordinary drawing dividers; but if these are used, it will be well to stick the points into little pointed tips of cork to avoid the sharpness and coldness of the metal. (An excellent, but more expensive, Æsthesiometer is made by C. Verdin, 7 Rue Linne, Paris, at 35 francs; for the description of an elaborate and very convenient one, see AMER. JOUR.

PSYCHOL. I, 552.) Something is also needed in experiment 6 *d* for rendering the skin anæsthesic.

1. The Sense of Locality. Touch yourself in several places with the same object, and analyze out, as far as you can, the particular quality of the sensation by which you recognize the place touched. This quality of the sensation is known as the "Local Sign."

2. Cause the subject to close his eyes; touch him on the fore-arm with a pencil point; and require him to touch the same point with another pencil immediately afterward. Estimate the error in millimeters and average the results for a number of trials, noting the direction of error, if it is constant. The subject may be allowed to correct his placing of the pencil if not satisfied with it on first contact.

3. Aristotle's Experiment. Cross the middle finger over the first in such a way as to bring the tip of the middle finger on the thumb side of the first finger. Insert between the two a pea or other small object. A more or less distinct sensation, of *two* objects will result, especially when the fingers are moved.

4. Judgments of Motion on the Skin. *a.* Subject with closed eyes. Resting a pencil point or the head of a pin gently on the fore-arm, move it slowly and evenly up or down the arm. Require the subject to indicate his earliest judgment of the direction. If the experiment is carefully made, the fact of motion will be perceived before its direction. *b.* Try a number of times, estimating the distances traversed in millimeters and averaging for the two directions separately. It will probably be found that the downward distances have been greater than the upward. *c.* Starting from a fixed point on the fore-arm move the pencil in irregular order up, down, right or left, and require the subject to announce the direction of motion as before.

Cf. Hall and Donaldson, Motor Sensations of the Skin; Mind, X, 1885, 557.

5. Rest the fingers lightly on the forehead and move the head from side to side keeping the fingers motionless. Almost the whole of the motion will be attributed to the fingers. Light tapping of the forehead with the finger we feel in the forehead more markedly than in the finger. With our own hand on our forehead we feel the forehead; with some one else's hand we feel the hand.

6. Weber's Sensory Circles. *a.* Find the least distance apart at which the points of the æsthesiometric compasses can be recognized as two when applied to the skin of the fore-arm. Try also the upper arm, the back of the hand, the forehead, the finger-tip and the tip of the tongue. Be very careful to put both points on the skin at the same time and to bear on equally with both. *b.* Compare the distance between the points just recognizable as two when applied lengthwise of the arm with that found when they are applied crosswise. *c.* Give the points a slightly less separation than that found for the fore-arm (crosswise) and beginning at the elbow draw the points downward side by side along the arm. They will at first appear as one, later as two, after which they will appear to separate as they descend. Something similar will be found on drawing the points from side to side across the face so that one shall go above, the other below the mouth. *d.* Make the skin anæsthesic with an ether spray and test the discriminative sensibility as before.

Cf. Weber's measurements as given in the text-books.

7. Filled space is relatively under-estimated by the skin. Set up in a small wooden rod a row of five pins separated by intervals of half an inch, and in another two pins an inch and a half apart. Apply to the arm like the compasses above. The space occupied by the five pins will seem less than that between the two.

8. Active touch, that is touch with movement, is far more discriminating than mere contact. Compare the sensations received from resting the tip of the finger on a rough covered book with those received when the finger is moved and the surface "felt of."

9. The time discriminations of the sense of contact are very delicate. Strike a tuning-fork, touch it near the bottom of the prong and immediately remove the finger so as not to stop the fork. The taps of the fork on the skin do not blend into a continuous sensation for the tactual sense, even when the vibrations are 1000 or more a second.

For sensations of minimal contact, see Ex. 22.

On the foregoing experiments, cf. Weber, Tastsinn und Gemeingefühl, Wagner's Handwörterbuch der Physiologie, Vol. III. pt. 2; Funke, Hermann's Handbuch der Physiologie, Vol. III, pt. 2.

SENSATIONS OF TEMPERATURE.

Apparatus. Two brass rods (6 inches long and 0.25 inch in diameter, turned down to a fine smooth point 0.5 mm. in diameter), paper ruled in mm. squares, menthol pencil (such as is used for headaches), centigrade thermometers, vessels of water at different temperatures.

10. Hot and Cold Spots. *a.* Move one of the pointed brass rods, or even a cool lead pencil slowly and lightly over the skin of the back of the hand. At certain points distinct sensations of cold will flash out, while at others no temperature sensation will be perceived, or at most, only a faint and diffuse one. Heat one of the rods and repeat the experiment. *b.* On some convenient portion of the skin mark off the corners of a square 2 cm. on the side. Go over this square carefully both lengthwise and crosswise for both heat and cold, drawing the point along lines 1 mm. apart, and note on a corresponding square of millimeter paper the hot and cold spots found, hot spots with red ink, cold with black. This time the points should be heated or cooled considerably by placing them in vessels of hot or cold water and should be kept at an approximately constant temperature by frequent change, one being left in the water while the other is in use. Break the experiment into a number of sittings so as to avoid fatiguing the spots; for they are very readily fatigued. A map made in this way cannot hope to represent all the spots, but it will suffice to show the permanence of some of them and possibly to show their general arrangement. *c.* Notice the very distinct persistence of the sensations after the point has been removed.

11. The temperature spots respond with their characteristic sensations to mechanical (and electrical) stimulation, and do not give pain when punctured. *a.* Choose a very certainly located cold spot and tap it gently with a fine wooden point (not too soon after locating it, if it has been fatigued in locating); or better have an assistant tap it. *b.* Thrust a needle into a well located cold point. Try both for comparison on an adjacent portion of the skin.

12. The temperature spots respond to chemical stimulation. Choose a convenient area, say on the back of the hand, and take its temperature carefully, allowing the thermometer to remain in contact with the skin as long as it continues to rise. Note the temperature and rub the skin lightly with a menthol pencil. After a little the sensation of cold will appear. Take the temperature of the skin again; it will be found as high or higher than before, in spite of the contrary sensation. The menthol makes the nerves of cold at first hyperæsthesic (so that they respond with their specific sensation to mere contact, and give an intenser sensation when a cold body is applied than do adjacent normal portions of the skin); afterward, however, all the cutaneous nerves become more or less anæsthesic.

Cf. on the foregoing experiments: Blix, Zeitschrift für Biologie, Bd. XX, H. 2. 1884. Goldscheider, Neue Thatsachen über die Hautsinnesnerven, Du Bois-Reymond's Archiv, Supplement-Band, 1885, pp. 1–110; Donaldson, On the Temperature-sense, Mind, X, 1885; and the literature cited by these authors. On the chemical stimulation of the temperature nerves: (Cold) Goldscheider, Ueber die specifische Wirkung des Menthols auf die Temperatur-Nerven, Verh. d. Berliner physiol. Gesell. 9 April, 1886, Du Bois-Reymond's Archiv, 1886, p. 555; (heat) Die einwirkung der Kohlensäure auf die sensiblen Nerven des Haut, Verh. d. Berliner physiol. Gesell. 25, Nov. 1887, Du Bois-Reymond's Archiv, 1888.

13. The temperature of the skin at any moment is a balance between its gain and loss of heat. Anything that disturbs that balance, causing increased gain or loss of heat, produces temperature sensations. It is common experience that a piece of cloth, a bit of wood, a piece of metal, all of the same temperature as the air that seems indifferent to the hand, cause different degrees of the sensation of cold when touched, because they increase the loss of heat by conduction in different degrees. If a paper bag be placed over the hand held upward, a sensation of warmth is soon felt, because of the decreased loss of heat.

14. Provide three vessels of water one at 30° c., the second at 40°, the third at 20°. Put a finger of one hand into the warmer water, a finger of the other into the cooler. At first the usual temperature sensations will be felt, but after a little they disappear more or less completely, because of the fatigue of the corresponding temperature organs. Now transfer both fingers to the water of normal temperature. It will seem cool to the finger from warmer water and warm to the one from cooler.

15. The intensity of the temperature sensation depends on the amount of surface stimulated. Dip a finger in cold water, then the whole hand. Notice the increase in sensation.

16. The fatigue of the temperature apparatus may produce an apparent contradiction of Ex. 15. Dip one hand entirely under cold water and keep it there for a moment. Then dip the finger of the other hand or the whole hand several times in the same water, withdrawing it immediately each time. The water seems colder to the finger or hand which is only dipped.

17. Hold a very cold piece of metal on the forehead or on the palm of the hand for half a minute. On removing it the sensation of cold continues though the actual temperature of the skin is rising. Sometimes fluctuations are observed in the persisting sensation. After contact with a hot body the sensation of heat continues in the same way, though the temperature of the skin falls. Goldscheider explains this result for cold in part by the persistence of the cold sensation in the manner of an after-image, and in part by the lessened sensibility of the nerves of heat; a similar explanation *mutatis mutandis* holds also for heat.

18. Extreme temperatures fatigue the sensory apparatus of both heat and cold. *a.* Hold a finger in water of 45° c., the corresponding finger of the other hand in water which feels neither cold nor hot (about 32°). After 10 seconds dip them alternately into water at 10°. The finger from the water at 32° will feel the cold more strongly. *b.* Hold a finger in water at 10°, the corresponding finger of the other hand in water at 32°. After 10 seconds dip them alternately in water at 45°. The finger from the water at 32° will feel the heat more strongly.

19. Hold the hand for one minute in water of 12° c., then transfer it to water of 18°. The latter will at first feel warm, but after a time cold again. The water at 18° first causes a decrease in the loss of heat or a slight gain but later a continued loss.

20. Fineness of temperature discrimination. *a.* Find what is the least perceptible difference in temperature between two vessels of water

at about 30° c., at about 0°, and about 55°. The finest discrimination will probably be found with the first temperature, if the discrimination does not prove too fine at all these points to be measured with the thermometers at hand. Use the same hand for these tests, always dipping it to the same depth. It is better to dip the hand repeatedly than to keep it in the water. *b*. The different surfaces of the body vary much in their sensitiveness to temperature. The mucous surfaces are quite obtuse. When drinking a comfortably hot cup of coffee, dip the upper lip into it so that the coffee touches the skin above the red part of the lip, or dip the finger into it; it will seem burning hot. Plunge the hand into water at 5–10° c. The sensation of cold will be strongest at first on the back of the hand where the skin is thin, but a little later will come out more strongly in the palm, where it will continue to be stronger as it approaches pain.

On these general temperature experiments cf. the works of Weber and Goldscheider already cited, also Hering, in Hermann's Handbuch der Physiologie, Vol. III, pt. 2, pp. 415-439. Fechner, Elemente der Psychophysik, Vol. II, pp. 201-211.

SENSATIONS OF PRESSURE.

Apparatus. Bits of cork. Weights for minimal pressure. (These can be cut from rectangular prisms of cork or elder-pith of equal area, and provided with bristle or hair handles and verified upon a sensitive balance. The prism should be from 3 to 5 mm. square. The handle is made by setting the ends of a piece of bristle or hair into opposite sides of the bits of cork or elder-pith, thus giving the whole something the shape of a seal ring, of which the cork is the seal and the bristle the band. A series ranging from 0.002 to 0.02 grams would be convenient; but for the experiment to follow is not necessary.) Two objects of equal weight, but unequal size; a large cork and a small one, made of equal weight by loading the smaller with shot, answer very well. Two metal disks of equal size and weight, *e. g.* dollar pieces; and two wooden cylinders three quarters of an inch in diameter and one inch long. Vessels of water at normal temperature. Weights for discriminative sensibility. (The last can readily be made by loading paper gun-shells with shot. The following would be a convenient series: One hundred grams (two of this weight), 102.5, 103.3, 104, 105, 106, 107. The Cambridge Scientific Instrument Co., St. Tibb's Row, Cambridge, England, manufactures a set, which can also be used for "muscle-sense" tests, containing 30 weights and giving ratios ranging from about one-fourth to one fiftieth, at a price of £5.)

On apparatus for sensations of pressure cf. Beaunis, Éléments de physiologie humaine, II, 579. Eulenberg, Berlin. klin. Wochensch. 1869, No. 44. (See illustration and description of Eulenberg's instrument in the Reference Hand-book of the Medical Sciences, Vol. I, p. 85.) Dorhn, Zeitschr. f. rat. med., 3 R., X, 337. Bastelberger, Experimentelle Prüfung der zur Drucksinn-Messung angewandten Methoden, Stuttgart, 1879. Jastrow gives in the American Journal of Psychology, II, 54, a very inadequate description of a very satisfactory instrument. See also notes on apparatus for the study of the Psychophysic Law to be given later.

21. Pressure points. Make an obtuse but extremely fine cork point (pyramidal in shape, for example, the pyramid a quarter of an inch square on the base and of equal height), set it upon the point of a pen or other convenient holder, or use a match whittled down to a fine point, or even a needle. Choose an area on the forearm and test for its pressure spots somewhat as for the hot and cold spots, but this time set the cork point as lightly as possible on point after point of the skin instead of drawing it along. Two kinds of sensation will be felt; at some points a clear feeling of contact with a sharp point will be felt, at others no feeling at all or a dull and vacuous one. The first are the pressure points. Goldscheider describes their sensations on light contact as "delicate," "lively," "somewhat tickling * * * as from moving a hair;" on stronger pressure, "as if there was a resistance at that point

in the skin, which worked against the pressure stimulus;" "as if a small hard kernel lay there and was pressed down into the skin."

The first are more sensitive to small changes of pressure, and, though with sufficient increase both give pain, their sensations retain their characteristics. They are closer together than the temperature spots, and harder to locate; and the fact that our most frequent sensations of pressure are from surfaces and not from points makes it difficult at first to recognize a pressure quality in their sensations.

Cf. Goldscheider, Neue Thatsachen über die Hautsinnesnerven. DuBois-Reymond's Archiv, 1885, Supplement Band, pp. 76-84.

22. Minimal pressure (or simple contact). Make weights that are just perceivable on the volar side of the fore-arm and on the tips of the fingers. Try also if convenient the temples, forehead and eye-lids. In applying the weights see that they are brought down slowly upon the surface of the skin, that they touch equally at all points, and that their presence is not betrayed by motion of the weight after it touches the skin. This can be done by using a pen-holder or small rod, with its tip put through the ring of the weight, for laying it on. Compare the relative sensibility found by this method with that found with Weber's compasses for the same parts, and note that the latter requires discrimination, not mere perception. The stimulus needed to produce this faintest sensation is known as the stimulus of the "Initial Threshold." See also experiment 28.

Cf. Aubert and Kammler, Moleschott's Untersuchungen, V, 145.

23. Relation of apparent weight to area of surface stimulated. Test with the equal weights of unequal size. The smaller will seem decidedly heavier.

24. Discriminative sensibility for pressures. Have the subject lay his hand, palm upward, on such a support as will bring his arm into a comfortable position and make his palm level, for example a folded towel on a low table or the seat of a chair. (The matter of an easy position for the subject is of cardinal importance in all psychological experiment, and is mentioned here once for all.) Lay in his palm a piece of blotting paper just large enough to prevent the weight from touching the skin. On this set a standard weight, *e. g.* 100 grams, and after a couple of seconds replace it with an equal weight, or one heavier (or one lighter) *e. g.* 106 grams, allowing that to remain an equal time. Require the subject to say whether the second weight is the same or heavier, (or lighter, if a lighter is being used). Find the weight that can be distinguished from the standard in 75 per cent. of the trials. The ratio between the difference of these and the standard is the index of the sensibility. The ratio will probably be about 5 : 100. It is best not to use both a lighter and a heavier in the same series; and with this method of testing the subject should always guess, if he cannot discriminate. Be careful in putting on the weights that the subject does not recognize a difference in the force with which they strike, also that suggestions by difference of temperature or by sounds made in selecting the weights are avoided. This method of determining sensibility is known as the "Method of Right and Wrong Cases." Cf. later experiments on the Psychophysic Law.

25. Cold or hot bodies feel heavier than bodies of equal weight at a normal temperature. *a.* For cold, take two dollar pieces, warm one until it ceases to seem cold; cool the other to 10° c. Apply alternately to the palm of the hand. The cold one will seem much heavier, perhaps as heavy as two at the normal temperature. *b.* For heat, take two wooden cylinders of equal weight, heat one to a high temperature by standing it on end in a metal vessel floating in a water bath. Apply the cylinders on end alternately to the back of the hand between the metacarpal bones of the thumb and first finger. The hot one will seem heavier.

26. Pressure evenly distributed over a considerable area is less strongly felt than pressure upon an area bordered by one that is not pressed. Dip the hand up to the wrist into water (or better still into mercury) of normal temperature and notice that the sensation of pressure is strongest in a ring about the wrist at the surface of the water, possibly stronger on the volar than on the dorsal side. The ring effect is unmistakable when the hand is moved up and down in the water.

27. Something of the refinement of the pressure sense in perceiving the unevenness of surfaces may be seen by laying a hair on a plate of glass or other hard, smooth surface and over it 10 or 15 sheets of writing paper. The position of the hair can easily be felt by passing the finger tips back and forth over the surface.

28. Something might be said in support of the hairs as independent sense organs. The finest respond with a distinct sensation of anticipatory touch, as it were, when they are moved, and probably this accounts for a part at least of the differences between the fore-arm and finger tips found in Ex. 21. Touch a few single hairs and observe the sensation.

Cf. Blaschko, Zur Lehre von den Druckempfindungen, Verhandl. d. Berliner physiol. Gesell. Sitz, 27 März, 1885, DuBois-Reymond's Archiv, 1885, p. 349.
On the general topic cf. Weber, *op. cit.*; Funke, Tastsinn und Gemeingefühl, Hermann's Handbuch der Physiol., Vol. III, pt. 2, pp. 289-414.

II.—STATIC AND KINÆSTHESIC SENSES.

This group of senses furnishes us with data respecting the positions and motions of our members, and of our bodies as wholes. It includes senses whose existence or efficiency is disputed *e. g.* (Innervation Sense and Muscle Sense) and others whose independence has lately been asserted (*e. g.* Joint Sense and Tendon Sense.) This embarrasses somewhat the selection of experiments, but those chosen are the ones that seem at present characteristic. Many of the weightiest psychological inferences depend upon the sensations of motion and position of the eyes. It seems best, however, to postpone the experiments upon these sensations to the section upon vision.

Recognition of the Position of the Body as a Whole.

Apparatus. A light wooden rod a yard long; a tilting board and straps. For the last a board seven feet long and 18 inches wide balanced across a saw-horse will answer. At one end a foot board should be fastened securely enough to bear the weight of a man when the board is in a vertical position. At the other end a plumb line and semicircular scale should be added so that the inclination of the board can be read off at any instant. For holding the subject securely upon the board when its inclination is considerable, and the subject is head downward it will be necessary to have a couple of shoulder straps passing over the subject's shoulders and fastening to stout screw-eyes screwed into the board itself or into the foot board, and perhaps a breast strap going about both the subject and the board.

Cf. Aubert, Physiologische Studien über die Orientierung, (translation with comments of Delage's Études expérimentales sur les illusions statiques et dynamiques de direction, etc.,) Tübingen, 1888, p. 41.

29. In this experiment it is especially desirable that the subject should know as little as possible of the purpose of the experiment. Cause the subject to stand erect with his back against a wall. Choose a point on the opposite wall about the height of his shoulders. Let him look at it, and then require him, having closed his eyes, to point to it as exactly as possibly with a light rod held symmetrically in both hands. Cause him also to hold the rod vertically and horizontally in the median plane; also horizontally parallel to the frontal plane. All these he will probably be able to do with much accuracy, or if, as sometimes happens, he shows a "personal equation," his error will be constant.

30. *a*. Cause the subject to repeat the experiment, this time turning his head well to the left after closing his eyes. Repeat, causing the subject to turn to the right. In both cases an error of about 15° will be observed, the subject pointing too far by that amount in the direction opposite to that of the turning of the head. The subject will be able to hold the rod vertical or horizontal without error. *b*. Cause the subject to hold the rod in what he thinks is a horizontal position in the median plane, when his head is thrown well back; when bowed well forward. Illusions like those observed above will result. *c*. Cause the subject to hold the rod in what he thinks is a horizontal position, parallel to the frontal plane, when his head is leaned to the right; when leaned to the left. Illusions similar to those in the previous experiments will appear. *d*. Repeat experiment *a*, but instead of having the subject point to the designated object, have him walk toward it keeping his shoulders square, his eyes shut, and his head turned to one side. He will walk more and more too far toward the side away from which his head is turned. In all these cases judgment of one cardinal direction in space alone is affected, the other two show little or no errors.

31. After some practice and with attention to the sensations, the illusion of Ex. 30 takes another form, namely, that the body has turned a few degrees in the same direction as the head. The subject can now point to the chosen object; but, if required to set the end of the rod against his breast so that it shall be horizontal and perpendicular to the line joining his shoulders, he will make an error of about 15° in the direction of the motion of the head. A similar illusion may be found for the other directions of head turning, if tried under proper conditions *e. g.* when hanging by the hands with the arms somewhat bent.

32. The illusion is due, at least in cases *a* and *b* Ex. 30, to sensations of the position of the eyes. As may easily be observed upon any other person, the eyes turn further than the head in the direction in which it is turned. From the eyes we judge the position of the head, and thus overjudging it,point too far in a contrary direction in trying to point to the required object. The illusions can be produced by motion of the eyes alone. *a*. Holding the head erect and taking pains not to move it when moving the eyes, turn the closed eyes as far as possible to the right or left and then try to point to some determined object. An error like that in Ex. 30 will be observed. Turning of the eyes upward or downward has a less satisfactory result. Instead of closing the eyes they may be kept open if an opaque screen is held close before the face. *b*. Repeat *a* and *b* of Ex. 30, voluntarily turning the eyes as far as possible in the direction opposite to that of the turning of the head. The original error will disappear or be found to have changed its sign.

33. Another set of illusions regarding the position of the body as a whole in space depend in large measure on the distribution of pressure on the surfaces of the body, the direction of pressure of the movable viscera and the blood. Secure the subject properly upon the tilting board, and have him close his eyes. Start with the board vertical, (head up).

The subject will probably announce that he is then leaning forward slightly. Turn him slowly backward and require him to announce when he is vertical (head up), when he is tilted backward at an angle of 45° from the vertical, when at an angle of 60°, when at 90°, when at 180°. Two classes of illusions will be found; angles of less than 40° will seem too small; those from 40° to 60° will be rightly judged; those beyond 60° will seem too large. The subject will probably say that he is vertical, head downward, when he is yet 30-60° from it.

SENSATION OF ROTATION.

Apparatus.—Rotation Table. This can be made well enough for the experiments given by laying a 7-foot board across an ordinary turning chair or screw stool without a back. The last must turn without appreciable noise or jar. Many of these experiments could be made perfectly well by twisting the ropes of an ordinary swing.

34. Lay the board across the stool and let the subject be seated upon it with closed eyes and blindfolded if necessary. Turn the stool slowly and evenly in one direction or the other. The subject will immediately recognize the direction and approximately the amount of rotation when the rate is as slow as 2° per second, or even slower. After continued rotation at a regular rate the sensation becomes much less exact or entirely fails. This fact has been generalized by Mach in the law that only change of rate, not continuous rotation is perceived. With an exception in the case of uniform rates for short times, this is accepted by Delage. After some pauses and short movements in one direction and the other, the subject may become quite lost and give a totally wrong judgment of the direction of motion, if it is slow.

35. Let the subject be seated as before. *a*. Rotate him a little more rapidly for half a turn, and then stop him suddenly. A distinct sensation of rotation in the opposite direction will result. *b*. Repeat, and when the illusory rotation begins, open the eyes. It immediately ceases. Close the eyes again and it returns.

36. *a*. Repeat experiment 35 *a*, letting the subject give the word for stopping. At the same instant let him incline his head suddenly backward or forward or lay it upon one shoulder or the other. The axis of rotation of the body will appear to change in a direction opposite to that of the inclination of the head, i. e., if the head is inclined to the right, the axis, seems to incline to the left. The feeling is as if the body were rotating in the surface of a cone in a direction contrary to that of the first rotation. The head dictates the apparent axis of rotation. The same illusion occurs if the head is inclined during the actual rotation and straightened at the word for stopping. Turning the head to right or left introduces no such illusions, because it does not change the axis of rotation of the head. The illusion comes out with very disagreeable strength when the rotation is rapid and the subject changes the position of his head during the rotation. *b*. Let the subject lie upon his side and rotate him rather rapidly till the sensation of rotation becomes faint or disappears. Then let him turn suddenly upon his back or upon his other side. The first brings the rotation about a new axis, and it is felt in its true sense, while the rotation about the previous axis is felt in its reverse sense; the second reverses the direction of motion completely and produces a correspondingly powerful sensation.

The change of the apparent axis of rotation with the change of position of the head points to the location in the head of the organ by which such sensations are received. For the experiments by which the semicircular canals are indicated as this organ see the literature cited below.

37. Another illusion of rotation (Purkinje's dizziness) is due to the

motion of the eyes. Let the subject whirl rapidly on his heels with his eyes open till he begins to be dizzy; while he whirls the objects about him will seem to be turning in the opposite direction. Let him then stop and look at an even surfaced wall while the experimenter carefully observes his eyes, picking out a fine blood-vessel, or some other clearly marked fleck or spot as a point at which to look. To the subject the surrounding objects will seem to continue to move in the same direction as before, i. e., in a direction contrary to his previous rotation; the experimenter will see the subject's eyes executing slow motions in one direction (in the direction of the original motion of the subject) alternating with rapid motions in the other. The subject himself may be able to perceive a corresponding irregularity of motion in the spots upon the wall at which he looks. The illusion rests upon the subject's unconsciousness of the slow motions of his eyes. It is not improbable that these eye motions and the sensations of attempted restoration of equalibrium in other parts of the body are reflexly caused by the disturbance in the semicircular canals. It should be noticed that this illusion is the exact reverse of that found with closed eyes in Ex. 35. There the subject feels a rotation of his own body contrary to that it previously received. If he was turned at first in the direction of the hands of a watch, on being stopped he would seem to be turning in a direction contrary to the hands. If these motions were transferred to objects about him, they would, during the rotation, seem to move contrary to the hands and after stopping in the direction of the hands. In the Purkinje experiment the motion of objects is not thus reversed.

SENSATION OF PROGRESSIVE MOTION.

39. So far as progressive motions do not partake of rotation the sensations which they give us are probably combinations of sensations from several different sources or sensory judgments based thereon. For them, as for the motions of rotation, the principle holds that we perceive changes of rate of motion, and not uniform motion; as long as the motion remains uniform we can by an effort of imagination conceive ourselves to be moving in either direction or to be standing still, except for the jarring. The apparatus for the study of these phenomena will be found in railroad trains and elevators.

On the sensations of this and the preceding sections, cf. Aubert, translation of Delage above cited; Mach, Bewegungs-Empfindungen, Leipzig, 1875; Brown, On Sensations of Motion, Nature, vol. XL., 1889, p. 449, ff.

MUSCLE SENSE, *Kraftsinn*.

The real muscular sensations are probably those of pain, fatigue and the like, and are of relatively minor importance for psychology, but the term "muscle sense" has been used to designate that sense by which lifted weights are perceived, and is here used in that sense.

Apparatus. Set of test weights somewhat like those used for the pressure sense, but less different one from another, (For example: 100 grams (two of this weight), 101.6, 102, 102.2, 102.5, 102.8, 103,3). Weight of 2 or 3 kg.

39. Discriminative sensibility for lifted weights. *a.* Let the subject stand at a table of convenient height. Place within easy reach of his right hand, and near together, one of the standard weights (*e. g.* 100 gm.) and a weight to be compared with it, either the other standard or a heavier (or lighter) one. Let the subject lift one after the other, taking care to lift them in the same way, at the the same rate and to the same height, and give a decision as to which is the heavier (or the lighter). Find the weight that can be distinguished from the standard in 75 per cent. of the trials. As before the ratio between the difference of these and the standard is the index of the discriminative sensibility. The ratio will probably be about 2.5 : 100. *b.* Repeat the experiment letting the subject lift with one hand the standard and with the other

the weight to be compared, keeping the same hand for each during each series of trials. Note the discriminative sensibility as before; the discriminations will be much less fine.

In these experiments the sense of pressure might be expected to co-operate, but when it is excluded or put at a relative disadvantage, the sensibility for differences of lifted weights is not diminished. Weber's method of excluding the pressure sense was to wrap the weights in pieces of cloth and lift them by the four corners together. The pressure on these corners can be changed at will irrespective of the heaviness of the weight lifted. Compare the descriminative sensibility found for pressure with that found for lifted weights.

40. Careful experiments on the method of such discriminations shows that the determining factor is the rapidity with which the weight rises as it is lifted. The following experiment is one of those upon which this conclusion rests. After having performed the second part of Ex. 39, compare the standard weight with a very much heavier weight, *e g.*, 2 kg., with all the circumstances of actual careful judgment. Practice this judgment thirty times, leaving a larger interval of time between the individual comparisons than between liftings of the weights compared. Then at once return to the smaller weights, giving the standard to the same hand as before and the weight to be compared to the hand that has just been lifting the 2 kg. Not only will the weight before just recognizably heavier seem considerably lighter than the standard, but also still heavier weights will seem so. This time the tests must be few, not more than three or four. If more should be desirable, practice the comparison, of the standard and 2 kg. weight again ten times before taking them. By the practice the nervous centres discharging into the the muscles that raise the 2 kg. weight become accustomed to a larger discharge than that required for the small weights and do not at once re-adapt themselves, but supply too great a discharge, the weight rises with greater rapidity than the standard and is consequently pronounced lighter.

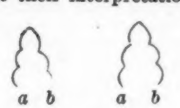
Cf. on Muscle Sense. Weber, *op. cit.*; Müller und Schumann, Ueber die psychologischen Grundlagen der Vergleichung gehobener Gewichte, Pflüger's Archiv, Bd. XLV, 1889, pp. 37-112; James, Psychology, II, pp. 189 ff.

Cf. also the experiments and references on the Psycho-physic Law.

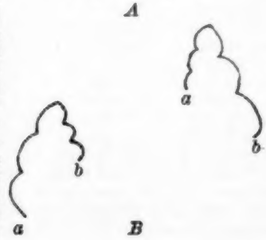
INNERVATION SENSE.

Apparatus. Black board and chalk.

41. The evidence most frequently offered in support of this sense is clinical and therefore beyond the scope of this course. Experiments like the following have been brought forward, but their interpretation has been disputed. *a.* Stand erect before the black board with the eyes closed and coat off, if it interferes with free motion of the arms. Draw with each hand a conventional leaf-pattern like those in the annexed cut drawing from *a* to *b* in both cases. In drawing try to make the lobes of the leaf of equal size, like those in Fig. *A*; draw each with a single simultaneous "free hand" motion of the arm, that is, draw each with a single volitional impulse directed equally to the two sides—the last point is important. First draw a pair of leaves beginning them with the hands before the shoulders at the same height; the result will be approximately like fig. *A*. Next draw a pair with one hand about a foot lower than before; the result will be like Fig. *B*. *b*. Bring the hands again



A



B

to the position used in drawing fig. *A*, and draw a pair of leaves having their apices right and left. The leaves will be symmetrical. Next begin with one hand about a foot farther away from the median plane than before and the other at it, but both at the same level. Draw as before; asymmetrical leaves will be the result. Repeat the drawing a number of times, sometimes raising or extending one arm, sometimes the other. In general it will be found that notwithstanding the intention to make equal movements of the hands, the motions of further extension in the extended arm and of further flexion in the flexed arm are too short and those in the contrary direction in each case too iong. The argument founded on this experiment runs as follows: We think that our hands execute equal movements, when they do not, because we are conscious of willing equal movements, and unconscious or only inexactly conscious of those actually made. If on the contrary we perceive motion of members by the skin, joint and muscle sensations that accompany their motion (as the opponents of the Innervation Sense believe) we ought to know the extent to which our hands are moved each time and not fall into the illusion that we find in these experiments.

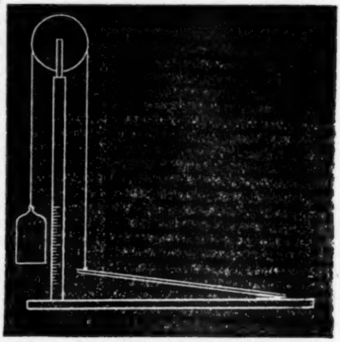
42. Lay the hand palm downwardon the edge of the table or on a thick book so that the last three fingers shall be supported and held extended while the thumb and first finger remain free. Bend the first finger considerably at both the inner joints, and hold it in position with the other hand. The finger tip is still movable as will be found on touching it, but it is anatomically impossible to move it voluntarily. When, however, the effort is made to move it (the eyes being closed) there is a sensation of motion, though no actual motion is possible.

43. Of experimental evidence against the Innervation Sense there is more. Müller's experiment (No. 40) seems conclusive against it; for if there were any sensation of nervous discharge, we ought to know that when we go from a very heavy to a light weight the discharge is disproportionate; but we do not. That the feeling of effort is of peripheral and not central origin is shown by such experiments as this of Ferrier's. *a*. Hold the finger as if to pull the trigger of a pistol. Think vigorously of bending the finger, but do not bend it; an unmistakable feeling of effort results. Repeat the experiment and notice that the breath is involuntarily held, and that there are tensions in other muscles than those that would move the finger. Repeat the experiment again, taking care to keep the breathing regular and other muscles passive. No feeling of effort will now accompany the imaginary bending of the finger. *b*. Lay the fore-arm entirely relaxed in the scale pan of an ordinary balance (or better still of a spring balance) and put in weights enough to compensate it exactly. Remain with closed eyes keeping the arm relaxed. It will after a little overbalance the compensating weights, showing that at first it was not wholly relaxed. An Inervation Sense, if we had one, ought to prevent such an illusion.

Cf. on the Sense of Innervation, Wundt, Physiologische Psychologie; I. 397, ff.; Sternberg, Zur Lehre von den Vorstellungen über die Lage unserer Glieder, Pflüger's Archiv, XXXVII, 1885, 1. Loeb, Untersuchungen über die Orientirung im Fühlraum der Hand und im Blickraum, Pflüger's Archiv, XLVI, 1—46, (but see also criticisms of James, Psychology, II, 516, and of Christine Ladd Franklin, Amer. Jour. Psy., II, 653); James, Psychology, II, pp. 486, ff.; Ferrier, Functions of the Brain, pp. 382 ff., (English Ed.); Funke, *op. cit.*

SENSATIONS OF MOTION, (*Joint Sense*).

Apparatus. Hinged board for passive flexion of the elbow. The accompanying cut will give some idea of the construction of such a board. The thin board on which the fore-arm rests (50 cm. long by 8-10 wide) is hinged at one end to the base board. At the other end a cord is fastened that runs over a pulley upon the top of a stout post. On the end of the cord a weight is hung to counterbalance the weight of the fore-arm. A scale (*e. g.* a piece of mm. paper) on the post near the weight enables the experimenter to read off the distance which the end of the arm-board is raised or lowered. It is essential that the hinge and pully work easily and without jar. The above is simply one way of accomplishing the result; others will occur to those for whom this construction is inconvenient.



44. Passive flexion at the elbow. Let the subject rest the fore-arm flat upon the arm board, bringing the elbow over the hinge, and close his eyes; raise the fore end of the arm-board slowly by pressing down upon the counter weight, and require the subject to announce when he first perceives the *motion* of his fore-arm. It is extremely important not to mistake the sensation of increased pressure or of jar for that of motion. With the dimensions given above, one degree of angle corresponds to about 8.7 mm. The same apparatus may be used for extension as well as flexion.

45. Active flexion of the last joint of the finger. The joint sensations of the fingers are less fine than those of the elbow, but are more convenient for demonstration of active flexion. Fasten a piece of straw, with court-plaster or otherwise, to the finger nail of the middle finger, and cut it off at such a length that the distance from the joint of the finger to the end of the straw shall be 115 mm. With that radius 2 mm. corresponds to about 1° of angular measure. Rest the hand on a thick book letting the last joint of the finger extend beyond the edge. Set up a millimeter scale at right angles with the straw. Close the eyes and make the least possible flexion of the finger at the last joint, having an assistant note its extent on the scale. Between one and two degrees will probably be the least possible voluntary movement. Close attention will, probably in both these cases, locate the chief sensation in the joint. For the more rigorous experiments required to show its character clearly and to prove its location see the following:

Goldscheider, Untersuchungen über den Muskelsinn. Du Bois-Reymond's Archiv, 1889, pp. 369 ff. and 540, also Supplement-Band, 1889, 141 ff.

SENSATIONS OF RESISTANCE.

Apparatus. Two or three kilogram weight and string. Vessel of mercury.

46. *a.* Hold the weight by the string so that it hangs a few inches above the floor, with the arm extended. Lower the weight rather rapidly till it rests on the floor. As it strikes, an illusion of resistance to further motion will be perceived. This is due to the unexpected strain put upon the muscles that lower the arm by the tension of those that have been holding the weight. The feeling of resistance is probably a

joint-sensation. *b.* Something similar is observed on pouring a quantity of mercury from one vessel to another.

Cf. Goldscheider, *op. cit.*

BILATERAL ASYMMETRIES OF POSITION AND MOTION.

Apparatus. Two medium sized corks. A millimeter scale at least one meter long. This can easily be made by pasting millimeter paper upon a smooth wooden slat. A convenient scale has a right angled triangular section. In use this stands upon the short side of the triangle, the long side is next the subject, the hypothenuse next the observer. The millimeter paper is pasted along the upper edge of the side next the observer.

47. Apparently symmetrical positions of the two arms. Hold a cork between the thumb and first two fingers of each hand. Close the eyes and bring the two corks together at arms length in the median plane before the face, having an assistant note the approximate amount and direction of the error. The corks should be brought together rather gently so as not to betray the character of the error to the operator, but the motions of the arms by which they are brought up nearly to contact should be free and sweeping. The error will probably be found rather constant in direction until the operator learns to correct it. Try bringing the corks together above the head, and also in asymmetrical positions.

48. Let the subject seat himself at a table with the millimeter scale before him. Set a pin in the middle of the scale and bring the pin into the median plane of the subject and make the scale parallel to his frontal plane. Let the subject place his forefingers on either side of the pin, and with closed eyes, try to measure off equal distances by moving each outward along the scale. Note the result in millimeters; for this it may be convenient to mark the middle point of the finger-nails with an ink-line. A constant excess in the motion of one hand or the other will be found. It is important that the subject should not open his eyes till his fingers are removed from the scale; for he will find it difficult not to correct his error if he knows its nature. The finger tips should rest lightly on the scale and the motions should be made by a single impulse; if they are too slow and the subject attends to his sensations of position, the errors will be small and uncertain. The greatest errors will probably be found for distances of 20 to 50 cm. from the median plane. The left hand generally makes the greater excursion in right handed persons not mechanics. *b.* Repeat the tests having the motions of the hands made successively instead of simultaneously. The constant difference between the hands will not appear. *c.* Operate somewhat as in *a*, but this time let the experimenter move one of the subject's hands passively while the subject himself tries to move the other at the same rate and to stop instantly when the passive motion stops. Try passive motions of the right as well as the left hand. The errors found will generally resemble those of *a*. *d.* Let the subject start with his right or left hand 20 cm. toward its own side of the median plane, and try to measure off equal distances on either side of that point, using the same hand for both distances. Indicate the point of departure with a pin as before and mark off with another the standard distance to be reproduced. Distances outward will be made too large, distances inward too small. In all these experiments with closed eyes we seem inclined to judge distance rather from the intention of equal motion and the continuance of motor sensations for equal times than from the actual peripheral sensations.

Cf. Hall and Hartwell, Bilateral Asymmetry of Function. Mind, Vol. IX; Loeb, Pflüger's Archiv, XLI, 1887, pp. 107-127, also Pflüger's Archiv, XLVI, 1890, pp. 1-46.

(*To be Continued.*)

CONTEMPORARY PSYCHOLOGISTS.

I.

PROFESSOR EDWARD ZELLER.

Professor Zeller was born in 1814. At the age of twenty-six he became docent of theology in Tübingen, in 1849 was called to Berne as assistant professor, and to Marburg as full professor two years later. In 1862 he was called to Heidelberg and transferred from theology to the chair of Philosophy, and ten years later came to Berlin as the successor of Trendelenburg, where he is now the leading teacher among six in his department. Here, in 1878 he was elected rector, receiving, according to the liberal German custom, a large fraction of all the examination fees. His chief publications are, *Geschichte der Griechischen Philosophie*, the first edition of which was begun in 1844, the fourth in 1876; *Vorträge und Abhandlungen*, in two volumes, 1875 and 1877, and *Geschichte der Deutschen Philosophie seit Leibnitz*, in one volume, 1873. He was the founder and for many years the editor of *Die Theologische Zeitschrift*, and has written shorter articles, to be found only in its pages. Personally he is a man of moderate height, almost alarmingly thin, sallow, of distinct, deliberate monotonous delivery, of genial but somewhat precise manners in personal intercourse. His lectures attract hundreds of students each semester. As a comparatively young man he married the daughter of F. C. Baur of Tübingen, and his family relations have been exceptionally happy. He, with several others of the prominent professors of Berlin, was a frequent and familiar guest of the family of the Crown Prince, Friedrich, and according to common report, was in an informal way the religious adviser of his consort. Next to Lotze he has been, no doubt, the most influential professor in his department in Germany. He has no system whatever, but has devoted his life to the critico-historical presentation of the views of others—a department of labor to which nearly half of all the philosophic writings and philosophic lectures in the German Universities are now devoted. In this work he is probably without a peer.

We shall first epitomize for our readers his remarkable summary of recent investigations in the field of comparative religions and the psychology of religion in the newly edited two volumes of essays, which contain his most personal as well as, perhaps, original views. In a laboriously compacted essay on the Origin and Essence of Religion he premises that philosophy, or psychology is the closest and most indispensible friend of religion. Doubt, which marks the important moment when we question or seek explanation about things which have surrounded us from childhood, is sure to lead to a deeper and purer faith, if it is manfully faced. The notion of God is not innate but is given perhaps necessarily, although always mediately, through experience, and thus rests upon the same sort of grounds and inferences as the notion of atoms, and is as scientific. In fact all intellectual possessions whatever are self-won, *we make* all which comes into our minds out of experience. All men irresistably infer that there are laws and that things are connected, not merely in time and space but inwardly; they not merely *are*, but *hang* together, and that not only in our ideas objectively carried over into experience but in themselves by the law of causation. Like all things else, causality itself must have a cause.

Thus at first as many substances are postulated as there are forces observed; later these forces are found so closely connected, their actions and reactions so poised and equipolated, the one by the other, that they must be thought as one. But this unity is, of course, as hypersensuous and immaterial as force itself. Consciousness in its essence is a union of the manifold, and hence by the very form of its knowing, its every object, whether cosmic or microcosmic must be a unity of the composite or the simple. Hence the necessity of inferring a primitive unitary force.

In illustration of these principles he proceeds to group a great number of facts gathered from popular psychology and mythology. From just such feelings as arise vividly in our own consciousness when we take the first spring ramble in the country, by the sea, or under the stars, from just such instinctive needs as we now feel in the thunderstorms, floods, in battle, famine or in sickness, and from the experience of common family and social life, not only all poetry and mythology but all religion have taken their origin. These are the feelings and needs they interpret. The strongest and most unwonted impressions drive us irresistably beyond the limits of our own ability or knowledge to supernatural causes. General forces are not deified. There are no gods of space or gravity. There was no interest in the beautiful but only in the pleasant and the fearful; and what was more fearful than night before man was possessed of fire. Because most of his instincts are satisfied, man assumes that somehow his wishes also must be, and so, when thought asks, fancy answers questions, though the answers are but changed facts. Thus the first religions are polytheistic and preserve a faithful record of the wishes, fears and hopes of prehistoric peoples whose spirits were wrought upon by nature and history with an intensity inversely as their knowledge of them. As long as no one dreamed that, e. g., even the sun and fire might be the same, no one thought of ordering this crowding and increasing throng of deities. But as men saw unity in the world and later as they learned to concentrate their own efforts upon some one paramount object, did first some group of gods become paramount, and later some one god was made king or president in the council of the others. Sometimes a tribe who had developed a national deity like that of the Jews induced others by argument or by the sword to accept it as their own. Perhaps monotheism was first the result of a philosophical critique, as among the disciples of Xenophanes, or again unity was favored or suggested by the all-enclosing arch of heaven and the apparent limits of the horizon and the visible universe. The visible always precede the invisible gods, and monotheism is always developed from polytheism. The personification of active forces is in fact the natural form under which the idea of cause is first represented. It is impossible to follow all the subtle associations of ideas by which the wind was represented as a breath, the lightning as God's spear, every bright or smooth stone as an amulet, the stars as living beings, by which a stick was thought to reveal hidden treasure, or a backward look to the left to cure sickness. Whether it was at first some fancied analogy or a rare but opportune chance sequence in time to which they owe their origin, they have possessed the mind of man for unnumbered ages, and hence, and because they minister, however unworthily, to a real need they are very hard to eradicate. As long as there is no postulate that all things are connected by laws, and in proportion as man is uncertain of his own position in an unexplored world where the most unexpected thing may happen to him at any moment, so long and so far is he anxious to get the unknown powers on his side by presents, wounds self inflicted, the offering of animals and finally of human beings and especially of innocent persons. Sometimes the deity is small and weak, or perhaps the priest, who even in the catholic church

holds a sort of conjuring power, has found some magic liturgy or ceremony of mighty and constraining cogency and the god may even be whipped or imprisoned till the wish is gratified or the fear allayed.

But religions must ever become more rational, i. e. immaterial. This process begins before all known developed languages, in the inference that soul and body are distinct, and that the *ego* is not a whole, made up of soul and body as subordinate unities. In the sight of a dead acquaintance and in dreams this distinction begins. The souls of men are at first located in the heart, head, bowels, liver, diaphragm, etc., and are weak and shadowy. The "he himself" is the body, as in Homer. They lead an unreal life under the earth, affecting the fruits of the fields, are figured as ghosts, manes, elves, and finally, in German mythology, survive as dwarfs, till the conception took more definite form in the idea of Hades. At first only chiefs and leaders had souls and these are invoked along with the sun and moon as in the worship of ancestors.

As long as their favor could be bought deities had, of course, no moral character. As hospitality, agriculture, commercial and family life, conceptions of regularity in nature, etc., were developed, the gods ceased to be awe or fear-inspiring (*unheimisch*) and influenced more directly the life men. Religion now began to grow in importance with the worth of the moral life. The lowest form of prophecy is the interpretation of signs. Faith is the child of the wish; then an audible voice must speak from the clouds; then men are inspired by a daimon, which is the inner oracle of conviction below distinct consciousness. They are suddenly and perhaps violently possessed by ideas, which wrap them in a dream, revery or vision, so new and so absorbing that the poet or musician dare not claim his work as his own. They did not know the inner process but were sure of its results, and thus inspiration was the best form which creative genius knew to give itself.

From the above principles of its growth we may infer why religion sometimes became largely political as among the Jews; why ancient systems lay great weight upon the cultus or worship and modern upon dogmatic orthodoxies; why religions must change; why the tension between the old and the new is proportional to the rapidity of this change, why all that cannot be harmonized to the new standpoints should, as Schleiermacher said, be allowed to lapse from the Christian consciousness and above all we can see that, as we should not study the Bible to know what Jesus believed but what we must,—so the worth of religion does not depend on how it originated but on what it does, just as the dignity of man and of science are not impaired by the conviction that one was developed from the ape and the other from astrology, alchemy, etc., or as the cogency of a man's logic is not prejudiced by the fact that a few decades ago we could use only baby-language. Not the first form but the historic principle is the essential thing. No man loses esteem for the German people of today by reading Tacitus or studying the life of the mediaeval Teutonic tribes.

A poetic race will emphasize the mythological element, a speculative people the dogmatic and a practical the active side of religious life, but Schleiermacher was right in seeking a deeper common principle. We must always reason from what the religious consciousness *says* to what it *means*. Dogma ignores scientific interests. Knowledge is no measure of the worth of religious life and is valueless for its own sake. Morality, very far from being the natural and implacable enemy of religion, as Feuerbach argued, is its chief, but not only constituent. As well argue against the use of fire on account of conflagration or against civic life because of corruption, or against judicial tribunals because they sanctioned the torture chamber, as to reject religion because of the selfishness, fanaticism and superstition which are ever found to attend it. Religion it not merely recognition of our duties as divine commands,

any more than the notion of God originated in the moral sense, but it includes everything which concerns the well being of man. It rests upon and is determined by the needs of social and individual life and especially of the *Gemüth*. Its cultus is the natural expression of a natural feeling and must evoke worthy frames of mind and all noble resolves. Does it bring joy and certainty into the life of the soul, does it increase the sense of personal happiness, rest, peace, etc., and not does it make us work successfully for the rewards of a future pay-day, are the questions? Can we dispense with the sense of our universal relationship and give up the postulate of the search for a unity of all things?

In his religious views Zeller has been greatly influenced by Schleiermacher whom he very justly terms "the greatest of all protestant theologians," a many-sided, Platonic mind, a true ethical genius who *must* preach," and who is best understood not by his avowed pupils but by those who, refuting his letter by his spirit, his later by his earlier works, have passed beyond his standpoint. His great object was to mediate between supernaturalism and rationalism, mysticism and empyricism, docetism and ebionetism, manicheism and pelagianism, to test the true value of all knowledge by the religious consciousness, to bring the culture of his time back to piety. In his earlier writings Schleiermacher argued that scripture became Bible solely by force of its own inward excellence, by the natural law of survival, in short that christianity does not insist upon being the only form of religion, but would prefer at any time to yield to a better should it appear. Not only all dogma but even Jesus himself, to whose person he attached such central importance later, but who never claimed to be the only mediator, were not indispensable to the Christianity characterized in the religious *Reden*. His philosophy was always for the sake of his theology of which it was only a broader form. God and the world, he says, are different expressions for the same worth, each unthinkable without the other. God is the essence of the world and not a will over it; hence there is no difference between His will and knowledge, between the possible and actual, between ability to do and performance. As providence is the law of nature, there can be no miracles, no origin of the world, no physical answers to prayer. Personal life is not the essence of the soul, but its phenomena, and the imperfection of the individual is but a part of the perfection of the whole. In short Spinozism is softened and idealized.

Corresponding to Kant's distinction between the sensory and the understanding, Schleiermacher distinguishes the organic from the intellectual function, the former as material and manifold, the latter as unifying, and each making and needing the other. All experience impels us to God, whom nevertheless we cannot know; yet he does not infer God to be unknowable, like Kant's thing *per se*, from the antithesis of our faculties, but because the nature of the ideas given in experience does not correspond to the God-idea which latter he thus illogically Zeller thinks presupposes. Our will he says fluctuates, but we vainly seek for the ground of will; while for Kant it is the will which first opens the intellectual world. The deepest problem—the relation between will and being—is found in personality, which is the appearance of the infinite spirit, and is the compendium of the universe. It must be developed to the fullest individuality between which and general laws there is no conflict. This noble romanticism, by which he strives to unite earnestness and scientific breadth and to rescue the abstract morals of Kant and Fichte from their subjectivity, makes our inmost nature the picture of the infinite, and personality the organ of knowing it. Here God and every moral principle is revealed, the knowledge of which we must work out into ever purer forms. Thus religion is feeling. Pure and perfect self-feeling, however, would be a knowledge of God

without the world, which is impossible because the God idea can never be freed from antithesis. But conception also gives a notion of God, hence religion is not over philosophy, and while the former should be universal, reason and criticism are also allowed free scope.

Over against an absolute power and causality no feeling but that of dependence, or of being determined, is possible. This form of feeling is originally given with personality, before all self-activity. We dare not say that we know its source, as substance or otherwise, hence there is no God-idea, save the vague *whence* of the feeling of absolute dependence, and all attempts to personify God are gratuitous. Religion originates naturally in us, but we must actively develop it, and it remains ever imperfect; hence there are always parts of sin as well as of grace in us. The former preceding the latter, as the life of sense precedes the life of mind. Because as feeling, it is the most individual, religious life most needs enlargement in companionship. It is aroused by intercourse and needs community. This is the basis of the church. Every experience or representation of the individual life by word or act, which arouses others to responsively produce the same state in themselves, is revelation. This is best seen in the expressive individuality of a relatively perfect type, and others are aroused to discipleship.

So far Zeller essentially agrees with his teacher; but when the latter proceeds to make Jesus typically perfect—(*urbildlich*) as a historical person by virtue of his special religion or God-consciousness, Zeller objects that Schleiermacher failed to prove the latter, and that Jesus in no authentic passage claims typical perfection. Jesus may be a perfect man, but he is not thus proven a God-man, and as such neither his person nor the dogmatic deduction from it are natural. With surprising critical freedom Schleiermacher, in the matured form of his system, urges that all Christ's acts and words merely reveal his personality and so far as this creates or wakens my religious life, or in other words makes grace outweigh sin in me, he may be called my redeemer. There is absolutely no substitute or proxy bearing of the penalty of sin. The church visible and invisible, or actual and ideal or typical simply aids men to reproduce the image of Christ, as a norm in their own lives. Thus in explaining the creative beginning of moral life in Jesus and the church according to profound and more or less intelligible psychologic laws, in exploring the essence of religion, and in transforming its traditions to the spirit of our times, in giving even theology a new ground in modern consciousness, and in deriving all from self-consciousness. Schleiermacher's work is incomparable and imperishable. But when, as dogmatist, he treats the gospels, and even John before all, as historical and labors with such painful ingenuity to pour his new wine into the old vessels which Strauss and Baur were so soon and so easily to shatter, he was not only inconsistent with the freedom of his own earlier position, but brought long discredit upon his religious philosophy, the most profound and quickening in modern thought. Zeller's attitude to Schleiermacher is thus somewhat analagous to that of J. S. Mill to Comte. While reproducing and developing the spirit of his earlier best period with ripened and condensed vigor, he rejects the tortuous scholasticism of his dogmatic and exegetical [according to Darner his best and most matured] systematizing, as worthless. Religion is well called a feeling, but to describe its content as one of absolute dependence is inadequate or at least misleading. It is as well the consciousness of absolute freedom in a pregnant Hegelian sense. No matter how philosophic the conception of fatalism may be made, it must ever be prejudicial to moral accountability.

In discussing the teleological and mechanical explanation of nature, which is perhaps the most fundamental question of religious philosophy Zeller urges that it is equally senseless and tasteless to conceive animals

as machines, the world as a huge time-piece, the mind as a body, attraction as caused by hooked atoms or to banish all notion of final causes as barren vestals on the one hand, and to explain trifles teleologically on the other. Neither can he agree with Plato and Aristotle that nature is to be explained in part mechanically and in part teleologically, nor with Leibnitz that the world as a whole is teleological and single phenomena mechanical. While the latter satisfies science it grants too much to metaphysics. All possible worlds had no struggle for existence in Gods mind, but the world as it is is the only possible form of its revelation, and is hence necessary. In a perfect nature the divine will and ability coincide with action. The world has no beginning or end in time. It was never without life and reason in some form. Because necessity is perfect and absolute it must be best conceived as imminently teleological, and the antithesis between the two applies only to its elements and not to the world as a whole. The need of the latter is only felt after man's acts have become plan-full, and if granted would require an infinite series of reasons, which at bottom would be, like the conclusions from logical premises, rather more mechanical than otherwise. Before matter, as space filling, moving, etc., can become an adequate logical cause of all things it must be conceived in a radically new way, while teleology is at most only one heuristic presupposition, and not a scientifically-grounded constitutive principle.

The development of monotheism Zeller considers as the most important of moral-theoretical problems, and among the Greeks the most gifted of all races, it is especially suggestive. The poesy of Homer and Hesiod depicts Zeus, the God-king, as subject to fate, surrounded by a turbulent and tricky aristocracy of deities, and although the protector of rights, as yet possessing no very moral character. His rule is far milder than that in the shadowy old dispensation of the Euminides. The poets were the first theologians, and it was they who reformed the crudities of the the early faith. Philosophy did not grow up, as since the christian era, in the service of theology. Xenophanes contributed the first monotheistic conception in describing men and gods as having one origin, and the Infinite One as being all eye, ear, thought, etc. From his keen irony *e. g.* in saying the Thracian gods have blue eyes and red hair, and that horses and oxen could they think and speak, would have quadruped gods like themselves, anthropomorphic polytheism never entirely recovered. The teachings of the sophists, which pervaded all ranks of society,—that we cannot tell whether the gods exist or not, that religion was the invention of shrewd legislators who sought in an appeal to fear the strongest sanction for their laws, or that the Gods represent those natural objects found to be the most useful, was followed by Socrates' conception of a unitary plan in nature and an all wise and good principle over-ruling all things. Yet he was by no means hostile to the popular religion, but believed in many Gods, who do all for the good of man, who must submit to and obey them, but also in a world-forming reason over all. The Eleatics believed in one only God, not in human form; the Cynics ridiculed the popular faith; the Skeptics declared it not proven, the Epicurians thought chance and necessity ruled the universe and that the gods led a life of placid repose, far off between the worlds and were worthy of unselfish veneration. Over Plato's eternal, changeless, ideal world, the Good rules supreme. It is the ground of all thought and being, giving to things reality and to thought truth. It is essential deity, towards which we strive in every act and thought, yet hard to know. It created and rules the world, is approached by purity of life, is not jealous of human happiness, is beyond feeling pleasure or pain in human acts. To this conception christian theology is immensely indebted. Yet Plato does not give up the idea of other visible gods. Stars, like the world, are incorporate

deities. Men must be trained by mythic lies to later abandon figurate and poetic for true thinking. Aristotle reasserts most of the same notions but adds that God must be a personal, active, first moving cause, etc., his providence imminent. The Aristotelean conception requires polytheism only for political ends. The Stoic pantheism which held that creative fire, reason and law could all be worshipped, also granted that myths were indispensable allegoric representation of eternal elements. But the reaction of skepticism which, from its extreme distrust of reason, came to long for revelation and which, even among the Jews after the Babylonian exile, admitted the doctrine of angels and devils to gratify man's polytheistic cravings, led, among the Greeks, to the notion of demons, which were only the old deities of polytheism, as the servants and tools of the supreme being. The commingling of races, led to the conception of the later Stoics, that all men are children of the same father, to the belief in the unity of God, and to dissatisfaction with any merely national god or messiah. The last stand of polytheism was made in the new platonic philosophy in its long but ineffective struggle with christianity, which, refuting its central conception of a descending series of beings emanating from the one perfect light, which was at last extinguished in inert matter, adopted it as a form of speculation. Thus, Zeller argues, Greek philosophy prepared the way, though somewhat esoterically at first, or Christianity, and supplied the elements for its subsequent rational development to an extent hitherto unsuspected.

Pythagoras, after premising that the stronger the impression made by any person or event the greater will be the mythopœic reaction, he infers that the sage of Krotona must have been a many sided, earnest and sagacious ethical reformer. He came from his native Lanos to southern Italy in a time peculiarly fitted for his work. The central doctrine of the society of which he became the centre was that of future rewards and punishments and the transmigration of souls, or that moral purification was the highest end of life. This and his asceticism were perhaps learned in the orphic mysteries. He taught music—or the art of the muses—and gymnastics, and that all things might be expressed in numerical relations. His followers refused to eat the heart of animals because it was the seat of life, and were buried in linen garments that the suffering of wool-bearing animals might be mitigated. They held their goods in common, made fidelity the chief virtue, and taught that the best should rule. Such are probably the facts. Within four centuries after the master's death his followers described him as a prophet, whose head was constantly surrounded by a nimbus, who called up storms, healed the insane, arrested plagues, called down an eagle from the sky, ordered a bear to cease eating flesh and was obeyed, was seen at two distinct places at the same time, was called by name by a river god, remembered his preceding life in which he was the son of Hermes, as in this of Apollo, heard and taught the harmony of the spheres, had made a visit to Hades, etc. Nearly all distinguished men in Egypt and the east, it was said, had been his teachers. The older his school became the more his young disciples were able to tell of him. He left no writings, but in the first century B. C., many ascribed their writings to him partly as a compliment and partly to win consideration for them, till several scores of volumes now bear his name.

The germ of the Roman religion he finds in the Latin-Sabine veneration of invisible spiritual beings in nature. The solitude of woods, the gurgling of springs, the crackling of flames, the gloom of forests, the phenomena of the sky, of growth of the seasons, suggested to the old Romans three classes of natural forces, heavenly, terrestial, and subterranean, which were poetically personified as gods instead of scientifically interpreted. The transiton from a fanciful conception, to a

matured ethical religion can be nowhere so fully studied as among the Romans, whose fundamental characteristic was awe of unknown forces, and constraint before supernatural influences. Hence their reverence for tradition their extreme care not to offend the gods by inauspicious chance words, by the neglect of the innumerable formalities which hallowed nearly every act of life. For centuries at first, like the Germanic races, the Romans had no or few images of the gods but later there were throngs of protective deities *e. g.* one for gates, another for hinges, one for doors another for thresholds, or again one for the cry of a new born babe, another for the father's acknowledgement of it, a goddess of the cradle, another who presided over the ceremony of naming, another was protectress from witches. There was one each for the child's food and drink, one which brought it from the cradle to the bed. Sacrifice was made to appropriate deities respectively that the boy's bones might grow rightly, when he first stood, walked, went to and came from school, to others that he might reckon, sing, be strong in body and in mind, etc. In the third and fourth century B. C., the influence of the religions of the north and south, especially that of Greece, began to be felt. First the mythology and rites, then the literature and later the Greek philosophy radically changed the popular faith and at last prepared the way for an easy transition to christianity. First the shallow Euhemerus taught that the gods were ancestors and Jove was the head of an old regent house. His doctrines were long influential. The epicurean deist Lucretius described the world as set free from the heavy oppression of superstition by philosophy. The Gods were far off and cared not for men. They could have no sex or age, the story of Iphigenia was an unmitigated horror. Scaevola declared that the religion of the poets was childish and often immoral and that of the philosopher abstruse and powerless, and held that religion was chiefly an art of the statesman, who must and ought to use it for political ends. That the *pontifex maximus* could thus hold dogma as nothing beside religious cultus without exciting antagonism is significant. Varro, the authority for most modern knowledge of the religion of ancient Rome, declared God to be the universe, especially the soul and reason. The public religion should be allegorized philosophy rather than the myths of the poets. Seneca's conception of a world-ruling wisdom, beneficent goodness, pious disposition, his description of deity, near, about, in us, was the highest form of Stoicism, in which it most nearly coincided with Christianity. Epictetus and especially M. Aurelius, to whom Zeller devotes a laborious essay, were far less emancipated from the popular faith. The former believed in Demeter and Persephone because men enjoyed their fruits, and because they restrained from wrong, and apparently never reflected that there is no error which may not do good at times, while the latter, too practical for the Stoic allegorization of myths, believed not only in dreams and oracles, but apparently in many foreign rites himself, and excused many other superstitions because they satisfied man's religious needs. Cicero held that faith in deity was deeply implanted in all men and was taught by the beauty and wisdom of the world, and that a pure heart was the best worship, and that whether or not the being of the gods could be scientifically proved, the natural religion must be strenously upheld as the chief bond of human society. After the republic the split between the doctrines of philosophy and the old Roman faith grew wider till the ancient gods lost their distinct individuality in the popular consciousness and the oriental monotheism of a denationalized Christianity readily absorbed all the purer and better elements of moral and religious culture into itself.

Nearly half of the first volume of the essays is devoted to a critical digest of the Tübingen school of theology of which Zeller is by far the

ablest and most philosophical and perhaps its most moderate living representative. The middle, half-orthodox party, which rested upon Hegel and Schleiermacher's attempt to reconcile reason and faith, and which never had any logical basis, was broken up by Strauss, Baur and Feuerbach and all its ambitious and domineering or weak and dependent members betook themselves to the confessional hyper-orthodoxy which was then favored by the reactionary German courts, and church and state fell largely under the leadership of dogmatic fanatics or impatient hierarchs. Though now a tidal wave of reaction has strongly set in, the desolation thus wrought in the head and heart may still be seen in the fact that, while other sciences have progressed, theology has been stationary or retrogressive during the last half century. On the one hand are the free religionists in Germany, shallow, tasteless, unscholarly, without thoroughness or method, negative and eminently unprogressive, and on the other ultra-orthodoxy of the Hengstenberg type ever elaborating its uncritical gospel harmonies or an exegesis of the patristic type which can put any meaning into or out of the scripture text, and well content with working out practical unionistic platforms for evangelical co-operation between trivially diverging sects. Both are alike unsusceptible, he says, to the great pressing needs of scientific theology, viz., the explanation of religion itself from its psychologic and of Christianity from its historical grounds. The latter problem is by no means finally solved by the Tübingen school. Baur, its coryphaeus, held that the last result of the criticism of the New Testament and other early Christian writers should and would be a noble and at the same time historical picture of Jesus himself. This, however, so far from being given by the negative residual methods of Strauss, could only be reached after the bias of each evangelist and apochryphists, the authenticity of every text, as well as its historical validity, and every personal, dogmatic or philosophical party influence of the age should have been weighed and tested. It was to this, in some sense preliminary work, which Strauss, by destroying the foundation of dogmatic supernaturalism, made possible, that Baur mainly devoted himself; and the goal which inspired him, but which he did not attain, must be striven for and reached by his method if Christian theology is to maintain a respectable position in the modern intellectual world. Man's desire for happiness is oppressed by a sense of his finitude, but the true religious consciousness reveals a higher and compensating happiness attained by the culture of purity of moral disposition. Man's elevation through the religious consciousness above the finitude of his nature, expressed as poverty of spirit, humility, simplicity, unselfishness, and the inwardness and absoluteness of religious life characterized by the doctrine of the fatherhood of God, something like this Baur thought would be ultimately found to be the fundamental idea of Jesus, conceived with intense realistic ethical genius and made a pressing and practical question by being boldly and sagaciously interpreted as the bottom meaning of a coming Mesianic reconstruction of the Jewish state. In his earlier Hegelian period Baur regarded Christianity as mainly a philosophical, but later as a purely moral problem. The incomparable influence which Jesus started in history consisted not so much in any novelty of his conceptions,—these are now traced to earlier sources; but in the nobility of his character; the force and purity of his personality were so great that a new moral and religious type of life was inevitable. He *was* the Messias in his own inspiring sense and not merely claimed to be. This, as every such conception must now be, in the absence of reliable or detailed historical information respecting Jesus, is as yet too general and vague, and must be, on the one hand elaborated by a sound and vigorous ethical philosophy into a wealth of needed moral power too long unutilized, (somewhat as Pfleidener has since at-

tempted, although the ethical genius of a Fichte is more adequate to such a work than that of Baur,) and on the other it must be verified and corrected by a deeper and stricter critico-historical study than even that of the Tübingen school has made.

One of Zeller's best essays is devoted to a characterization of his teacher, F. C. Baur, who, in his uneventful home-staying life, his slowly ripening nature, his amazing industry and perseverance, in philosophic, critical tact and vigor, in the growing importance and initiative power of his work, is aptly compared with Kant. His temperate mind could hold an important question open for years, sifting and weighing evidence with piety to every suggestion of fact, and so honest and *anima candida*, so without hyper-self-consciousness that he seemed like the noblest of the old reformers, while his moral sensitiveness was so acute that he was more grieved by lack of thoroughness or truthfulness in the work of his pupils than by the bitterest attacks of his opponents. He lived in and for his work, but could always preach edifyingly to the *Gemüth*, and his nature was profoundly religious and pastoral. His school, which has revolutionized religious opinions throughout Germany, Holland, Switzerland and even in protestant France, and has found many points of access even to English and American thought, is unlike the liberalism of deists, encyclopedists, etc., Schiller, Strauss and Feuerbach, in that it was founded by professional theologians and by men of deep personal piety., It simply drew conclusions which hovered in the intellectual air and which every one who thought logically must infer. It showed the time its own images and in urging that the New Testament was not pure history and not supernatural it only applied the critical methods which had almost revolutionized our knowledge of antiquity and its literature. Every one has smiled over the forcing, torturing and tasteless methods of the old German rationalism which explained away the miracle of Cana as a wedding jest, the fiery tongues of pentecost as electricity, the resurrection of Jesus as recovering from a trance, and declared that Paul at his conversion was blinded by lightning and was cured by the natural effect of the shock of an old man's hands, that the fetters were shaken from the hands of Paul and Silas in prison by an earthquake, that Jesus, though seeming to walk, on the water, really walked at its edge on the shore, etc.; in short, that oriental imagery and the reference of mediate natural processes immediately to God, which, though the exorcising supernatural, makes scripture none the less credible, but in an altered sense, even this had its effect upon the then current method of orthodox interpretation because it was no less tortuous and tasteless, as is perhaps best seen in the church history of Neander, who, without abandoning a single miracle or wavering on the doctrine of inspiration, which makes all Bible criticism impossible, yet loves to break off the points of the strongest miracles and is constantly conceding to the rationalistic methods, and capitulating to the *Zeit-Geist*, Bruno Baur, who has since declared the Tübingen school too conservative and apologetic, and been removed from his professorship, and who deduced Messiahship, resurrection and other evangelical *motives* from abstract dialectic principles ignoring or denying the existence of an historic Jesus. Marheinike, who made Bible texts into many-sided scholastic formulae, and Göschel, who all but identified philosophy and scripture, were alike unable to see the necessity or accept the results of such minute and painstaking researches as those of Baur.

First of all it must be borne in mind that the sense of literary property during the early Christian centuries was as undeveloped as any socialist could desire. Plato and Xenophanes put their sentences into the mouth of Socrates, [perhaps somewhat as a modern theologian states the *true Bible doctrine*, although in quite other than scriptual terms]. To present or develop the views of another, to win attention,

to produce immediate effect, to seek shelter from criticism behind a great name, personal modesty, piety to a beloved teacher to whom now a days a volume would be dedicated—all these motives of apochryphal fabrication were so common that a moralist must be as *naive* and devoid of historic sense to raise the scruples of a modern conscience here, as to apply the laws against stealing in a modern statute book to the constitution of Sparta. The well proven cases of pseudonymous authorship in ancient times, many of which Zeller instances are, extremely numerous. Baur's conclusion that the gospels and the greater number of the epistles are unauthentic, of later origin and mainly records of violent partisan controversies which rent the earlier Christian party from its beginning, opens the most interesting and classic of all ancient literature to the use scholars, elevates of and frees the intellectual life of the age, to a degree to which only the work of modern science can be in any degree compared. It was his special endeavor to discover the bias or tendencies of the early Christian writers. In an age when men believed what pleased, interested, or edified them, whether that Homer argued for the Jewish sabbath, that Orpheus sang of Abraham, Moses and the ten commandments, or that an old or hardened heathen was converted by a relic stealthily laid under his pillow by night, in chiliasm, or that new records of the life of Jesus written by apostles were suddenly discovered at opportune polemic moments, and when credentials and criticism were all but unknown, the chief task of the historian is to seek and define the tides of party feeling and prejudices, the currents of men's wishes, ambitions and hopes, and occasionally political relations and the ground traits of individual character. These with traditions and sagas as material for a mythopoeic fantasy in a most agitated age of persecution and millenial expectations must be *controlled* before objective history can be reconstructed.

It was hard for the personal disciples of Jesus and the Ebionite party gathered about them to uphold his tenets against the dominant Pharasaic sect after their leader had been executed as a seditious agitator, but it was still harder for them to see Paul, who had never known Jesus personally, so successfully propagating his teachings, as not only independent of, but actually irreconcilable with Judaism, among Gentile races and even declaring that by it Jews were freed from their own laws. The conservative wing of the early Christian party, which held that Jesus could be the Messiah of the Jews only, and that the mosaic rites and laws were still binding as a propædeutic of Christianity, regarded Paul as an interloper who really designed to use the large collections he was making ostensibly for the church at Rome to buy the gift of apostleship. He is again even described as a conjurer who represented himself inspired till Peter exposed him. To define and defend his universalistic view, viz., that Christianity simply set men in right relations to God, Paul composed the letters to the Galatians, the Corinthians and especially that to the promising and hitherto neutral church at Rome. Meanwhile hard pressed and perplexed by the vast discrepancy between the actual low-born Jesus and the splendors of the Messianic kingdom of popular and patriotic hopes, the disciples had come to expect that he would appear again—an event by no means unparalleled in Jewish story and inaugurate a new kingdom of indescribable magnificence. Nero, the anti-Christ too, it was rumored was not dead but had escaped and would come again with oriental armies, and new wars and persecutions would most severely test the fidelity of the faithful. In this condition of things the book of revelations was written by John as a manifesto before its decisive struggle after which the millenial new Jerusalem, with Jesus as king, would fill the earth. Thus read it is no longer a puzzling riddle-book, but most historical and authentic, in fact the only book in the New Testament written by a

apostle-disciple. The old Israelite expectations are all to be fulfilled in the wonder world of the re-appearing Messiah. Those who claim to be apostles but are not, together with the hated doctrines of the Nicolaitanss—bitter allusions to Paul and his teachings—are to have no place in the new theocracy, with its walls of jasper, its streets of gold and its tree of life. Thus too the most phantasmagorial dream of Jewish patriotism is successfully used to save the forlorn hope of a leaderless and losing cause. The controversy between the Pauline and the Jewish Christians was long and bitter, and colors, if it did inspire, most of the books of the new testament. The twelve apostles are paralleled, by the seventy co-workers of Paul. The Petrine party elaborated the Samaritan, Judean, the Pauline, the Galilean activity of Jesus. Peter is even represented as the founder of the first heathen church at Antioch, and is made to go to Rome because Paul had been there. James repudiates Paul's doctrine of justification by faith, urges that even devils may believe, and represents Jesus, perhaps his brother, as an ascetic Essenc with long hair, and as abstaining from flesh and wine. In a word, Paulinism, which dispensed with offerings and with circumcision, and denied that the only way to the new faith was through Judaism, stands for the freedom of wisdom and mature manhood while the Jewish Christians argued for a status and moral regimen of adolescence.

Meanwhile both parties were persecuted alike, both were represented in nearly every church, practical, administrative, unity became more essential as the church began its immense organization, while old passions and prejudices only faintly survived in a new generation. In the second century a conciliatory desire to save the effects of the work of both wings is manifest by accommodating, and often even transforming their destructive tenets. Thus Acts written in the second century and based perhaps on notesof Paul's traveling companion, and Luke, though both written with unmistakable Pauline drift are very conciliatory, Colossians ends with complimentary mention of a list of Petrinc worthies, while like Ephesians its Paulinism is very tempered. The first epistle of Peter makes surprising concessions to Paulinism. On the one hand it was apparently granted that Paul was too intricate and speculative, and that faith alone was not enough for salvation and on the other it was necessarily acknowledged that the wall of partition, between Jews and Gentiles was broken down in fact, and the vast number of non-Jewish Christians were taken into fellowship. Thenceforth all traces of primitive discord were carefully scored away, and the energies of the church were free for the work of practical and dogmatic development and defense. As the church grew, all parties united to elevate the conception of the person of Christ still higher, a convenient point for dogmatic unity in zeal for which old discords might be forgotten, until at last even the Messiah idea with which it had become identified in the first century was not exalted enough for the head of a church that had its stronghold in the capital city of the world, and was destined to become universal, and of a hierarchy so rapidly growing in influence and self-conscious dignity. The son of David gradually became with the growing influence of the ultra-Pauline Gentile element and the Alexandrian gnostic-philosophy not merely the heavenly pneumatic man, the new Adam, but the pre-existent, creative Word. The gospel of John, (A. D. 170-180) which is not historical, but represents the maturest and best points of the work and teaching of Christianity up to the period of its composition, which quietly appropriates the serviceable elements of the dangerous heresies of gnosticism and montanism, and shows no trace of heirarchism in the church, marks the point where the history of primitive Christianity ends and that of catholicism begins. The charm of the Johannean image of Jesus, so pure, so exalted and almost femininely delicate, so harmonious that his inward peace was

undisturbed by conflict and sorrow, and so free from all earthly limitations is unprecedented among all ideal personalities hitherto offered to human contemplation. The Johannean gospel not only reconstructed the previous Christian history from its new and tranquil stand-point, but represents the highest theological development of the first period of Christian history.

In an essay entitled Greek and Roman Prejudices against Christianity, Zeller shows that while the reign of Alexander and the Roman Empire had prepared the way for the outward spread of Christianity, the popular Stoic philosophy, which taught that all men were brothers with equal rights aud duties, and subject to the same moral law, which instead of faith made ethical temper the saving principle and divided mankind into two great classes, the fools and the wise, instead of the redeemed and the lost, and which longed for the "birthday of eternity," an entrance into the "great eternal peace," prepared the mental soil for the reception of Christian doctrine. The popular heathen notion was that the Christians, if not all Syrian barbarians, were yet atheists, criminals, who perhaps cooked and ate children, prayed to a God with an ass' head, were the worst and most unpatriotic citizens, and in fact enemies of the human race, so that Nero found no difficulty in circulating the report that it was they who had fired Rome. Pliny thought their creed in itself a harmless superstition, but believed their stiff-neckedness in refusing to adore images of the Gods and the Emperor, and in violating the laws against making proselytes should be punished. The mild M. Aureleus persecuted them because he deemed the pertinacity of their creed,—so unlike his all sided toleration and uncritical eclecticism, dangerous to the dicipline of the state. Lucian said the sect was composed of pitiable and deluded disciples of an arch sophist. The platonic Celsus argued that Jesus had stolen and disturbed philosophic doctrines which he could not understand, was of dishonorable birth, and a conjurer. Greek joyousness and Roman pride had only contempt for a religion designed for the sorrowing, oppressed, weak and guilty. The Neoplatonists revered Jesus, but one inquired like Reimarus, why, if salvation was through him, he appeared so late, and urged that if Peter and Paul could disagree about fundamental tenets his doctrine must be very uncertain, another thought Jesus did too few miracles to be really a god and proved that Apollonius did far more, while Julian, believing it was impossible for all men to have the same religion, argued that all noble men and great deeds in the world had come from heathendom and forbade Christians to teach the ancient literature.

In the saga of Peter as Roman Bishop the ultra ebionite view of Paul, which described him under the name of Simon the gnostic Samaritan sorcerer, who, after he had been exposed by Peter in Palestine came to Rome, where, by his arts and by the aid of demons, he had won great honors and many followers, is the ground motive. Later when Romish canonists sought to derive the power of the popes directly through Peter it is said that the latter followed Simon to Rome. The Jewish legend dishonored Paul whom the catholic party would honor, hence he is now distinguished from his double and made to join Peter in opposing Simon, and both Paul and Peter it was said died in Rome. Later Peter alone is made the first Bishop of Rome and thus the greatest work of Paul's life is accredited to the hostile apostle of the circumcision.

Much importance is ascribed to Schwegler's work on Montanism and the Post-apostolic age wherein it is concluded that Christianity assumed at first to be nothing but a more complete form of essenic Judaism, and that the autonomy and universality which Paul attempted to give it, transformed and dejudaized it materially less than had been generally supposed.

In the Platonic republic Zeller sees not only a significant ideal and prophecy impossible of realization, despite Plato's unreserved belief in it, and not only a product of the time, when, after the Pelloponnesian war, thedangers of individualism, the greed of riches and party strife seemed to show that men could not be trusted with their own development, but especially a type of society which has been no less than a germ for the organization of the mediaeval church. Instead of the philosophers who were to rule absolutely in the Platonic republic, are the priests, instead of the warrior cast, the temporal powers, instead of community of goods which was an early Christian ideal voluntary poverty of goods or of spirit and the mendicant orders. Community of wives, which was recommended to restrain not to satisfy desire, is paralleled by celibacy that monks may live all for the church. Both the ideal and the actualized system rest upon ethical dualism and teach that suffering here will be compensated in a future life and both assert a divine leadership of the state. The republic, like the kingdom of God, is an institution for training men in virtue. The church on the other hand does not so absolutely subordinate the individual to the community and the spirit of universal fraternity is widely contrasted with the casts and the national exclusiveness of the republic. While Plato would class modern theology, so far as it does not coincide with philosophy as mythology, and would be able to find in modern universities no suitable philosophers for rulers, and would be incensed at the modern political romances, wherein private interest is satisfied instead of annihilated, he must nevertheless be counted as one of the most important predecessors of organized Christianity. Much space is devoted to show that this was not the result of mere analogy but was history and that Plato's conception, at first too spiritualistic to be popular, had passed into the general culture of the day.

This matured and moderated digest of the Tübingen school so briefly and imperfectly epitomized and digested above, records, we believe, the most important achievement of the historic critical method. It affords the general terms of a suggestive and edifying solution of the most intricate and also the most obscured of all historical problems,—a problem not of one sect and race or century, but of commingled nationalities of contending political and philosophical, religious, partisan and personal interests. The facts were so inaccessible and so metamorphosed in this long contest, that only the most patient and conscientious research coupled with amazing psychologic insight and tact was able to reconstruct them at last after ages of misconception, with so high a degree of verisimilitude that the most distinguished of Roman historians, whose essential impartiality cannot be denied, declared that several years ago no German scholar under forty-five had thoroughly studied the Tübingen writers without being in the main convinced by them. There will long be many to fear that moral restraints may be practically weakened if scripture is proven uninspired in the old sense, or if miracles are disallowed, just as the Emperor Julian feared that classical literature would be ignored and perhaps lost if faith in the ancient gods was destroyed. This is, without doubt, sometimes the case among the young and the undiscriminating. But on the other hand it is only thus now made possible for men of thorough modern culture and moral self-respect to call themselves Christians if they will, and to be so in mind and heart in a sense deeper and larger than many conventional churchmen comprehend, and even if they see fit and hopeful occasion, to urge friendly even though misconstrued aid in ameliorating the narrow severity in faith and life, and in sustaining and reforming church organization, as a right by no means invalidated by stricter modern definitions of the Christian name but rather new vested by the supreme sanction of a positive and adult moral understanding. Myth is a deeper

and broader expression of humanity's common nature and needs than reason itself has yet attained. It is never the utterance of the mere individual, but is the *logos*, or over-soul of the half-unconscious moral instinct of a race or an age. It is never bound too closely to details of place or time. These only hinder or embarass its rare and strange moving and edifying power. In its noblest scriptural form,—Biblical in the classic sense wherever found—it comes most clearly and directly home to the *Gemüth*, takes men out of their own selfish personal lives, and raises, purifies and broadens their motives and feelings and purposes, as nothing else does. How to make it most effective for good is a problem which *homiletic* art has perhaps not yet finally solved.

The "Tübingen men" in Germany have grown now inactive and retrospective, and even Zeller is somewhat prolix and boastful in his recapitulations, and yet not only is their critical work incomplete, but its practical deductions, (the last consideration of a German savant) because left to be drawn in a negative, popular and superficial way, have been often sadly injurious instead of most helpful as they should be to the cause of religion and morality, and the German capital has grown perhaps more unconscious of the existence of religion and its institutions than any city in Christendom. Far from assenting to any ultra theories as *e. g.* that of Rothe—that the modern state more than the church expresses the essence of Christianity,—we cannot deny that the latter has grown far too consubstantial with our social, moral, intellectual and æsthetic life and development to be eradicated by any violence, or even to be intellectually distinguished and traced through all the long and subtle associations, by which it has become ingrained in our inmost psychic character. By being proven the oldest of all historic categories, and rooted in the earliest written records, instead of a supernatural graft upon an old and decaying trunk, it challenges the reverence of science itself as the most important problem of popular (Völker) psychology. by contributing to the experimental solution of which all known civilized races and ages have become in a noble philosophic sense organically united. As the modern musical scale, and the masterpieces composed in it are not endangered by the proof of its mathematical inaccuracy, its rude empirical origin, or by the suggestions of improvement, as the modern state is not lost to socialism by the demonstration that all values originated in the ten fingers of the working man, or that the rights of bequest and of absolute private ownership are, so to speak, recent habits, resting upon a series of accidents and misconceptions, so the Christian church is by no means essentially or permanently weakened by being compelled to relinquish its belief in miracles, inspiration and an incarnate deity for more historical conceptions of its origin. It is only another reformation that impends, as radical, possibly, to the more assumptive and unreasoning of modern Pharisees as was the new dispensation of Jesus itself, but only salutary to every true religious interest.

A brief notice of some of Zeller's less important essays will perhaps convey a better idea of the range and minuteness of his learning and of the acuteness of his critical power. In his defence of Xanthippe he reminds us that the young wife of an old man who could humorously boast of the advantage of ugliness like his over the classic Greek type of beauty, that the bridge of his nose was low, that with one of his prominent eyes he could look directly into the other, his mouth so large that he could save much time by eating faster than others, and his lips so thick and soft that he could give and receive the sweetest kisses, and whose ponderous body was the type of Silenus, might be excused for not being proud of the most monstrous among all the handsome Greeks for a husband. Moreover he would seek no office, lounged all day on the streets and in the public marts talking with tailor, shoemaker and

even *heterai* about the dialectic conception of their profession, and although so poor that wife and husband had but one outer garment between them so that one must stay at home when the other was out, would sometimes stand all day in one spot lost in revery, ridiculed by boys and comedians, and at last come home, old and fat as he was, to practice a dancing lesson for which, perhaps, he had paid his last *heller*. Moreover the suspicion that he had married her as a discipline in patience would hardly have been delicate and flattering to a woman's nature. When she came sobbing with a child in her arms to see him for the last time in prison before the fatal draught of hemlock, and severely looking at her he ordered Krito to take her home, and when she had been removed screaming, he calmly began a philosophic discourse. Possibly Xanthippe threw dirty water upon him, attempted to tear off his garments in the market place, overthrew his table and trod upon a cake that had just been sent in, no one knows from whom, and perhaps Soccrates consoled himself that she never kicked or bit him, but more probably these are unsalted inventions of lively Greek gossips or chronicles to make the name of Socrates brighter by contrast. She was probably no worse than many a modern woman would have been with her provocation.

Very readable is his characterization of Alexander the Paphlagonian imposter and Peregrinus the enthusiast. The former was famous for his beauty, and lived in the time of Trajan. Planning to found a new oracle at Abonuteichas, he buried and caused to be found, brazen tablets announcing that Askulepios and his father Apollo were about to remove thither, caused it to be announced that he was the grandson of the former, and later appeared himself in purple with the sword of Perseus in a feigned exstacy and with artificial foam flowing from his mouth. Throwing aside all his garments he showed the assembled multitude a young serpent in a broken egg shell, and a few days later an immense artificial snake with a human face, with eyes and mouth worked by invisible hairs, which he declared had grown from the little one and was a new God, Elycon, and from whom he would receive divine messages. Sealed letters were sent, and if they could be opened and sealed without suspicion were returned with answers written beneath every question. He hired a *claque* in distant cities and finally in Rome who reported the most astonishing miracles—hidden treasures and thieves discovered, the sick healed and even the dead raised. Messengers were bribed, difficult questions generously referred to the priests of other oracles, until at last Rutilinus, a man of high standing in the Roman court, like another Zöllner, fell completely into his net, and he became the fashion in the imperial city to which he graciously offered his protection against pest, conflagration and earthquake. He became immensely rich and made it dangerous for rationalistic epicureans or for Christians to attempt to expose him, and died at the age of seventy with undiminished fame.

Peregrinus of Pazium gave his fortune to his townsmen and traveled in the east where he learned the "rare wisdom" of the Christians who, it was said made him a bishop. Later he appeared in Rome as a cynic, anathematizing all the world, and especially the Roman emperor Antonius Pius, who banished him from the city limits, beyond which he lived in a hut and attracted many young men by his philosophical discourses. He afterwards went to Greece, and when no one took further notice of him, announced at the end of the Olympic games that at the end of the next festival he would burn himself alive. When the time came he had an immense pile erected, made a long harangue to the curious crowd, enumerating all the privations and sufferings which he had borne in the service of philosophy, declared he would die, like Heracles, to teach men to dispise death. As, contrary to his hopes, no one interfered, but rather when a single voice cried "save thyself to the Greeks,"

the crowd vociferously exhorted him to courage and the speedy accomplishment of his purpose, after adjourning the act till another night "that the moon might also see it," clad in the Cynic uniform, casting a handful of incense into the flames and commending himself to the spirits of his ancestors, he walked tremblingly and pale into the flames and was seen no more.

In "A Strike in Rome" Zeller discusses the variously recorded story of the origin of the Roman festival quinguatrus. The pipers, it is said, vexed by certain restrictions of their prerogatives, withdrew to a man to Tibur and occasioned thereby great distress in Rome. There could be no festive sacrifices to the gods, no religious processions, no marriages, no burials. The senate in vain tried to induce the irate musicians to return, and only after they had been made drunk at a feast given in Tibur and brought home in wagons did they consent, if all their ancient rites were restored, to resume their duties as before. This is compared with the legend of the origin of festivals of *carmenta*. The Roman matrons of old had the right to ride in carriages, of which they were deprived by the senate. They all swore to bear no more children till the privilege was restored, which the senate hastened to do. Both these tales, the former of which has been hitherto undoubted, Zeller argues with great ingenuity are instances of the ætiological sagas so common among the Romans and utterly without historical foundation.

In characterizing Fichte as a politician we are told that he possessed the very rare combination of great scientific acumen and culture, with immense vigor and sensitiveness of moral character. It was the substance of his philosophy that the will of the individual created not only his own character, but his own world, and that individual action and development might be free and unhindered, was the ground motive of his life. The true state itself is only a three-fold compact of the sovereign people who can therefore never rebel. Its business is solely to protect men in their rights. To this end they must oversee all departments of work in every detail, and cause every one to be remunerated according to his services. This view has made him a favorite with the modern socialists. It alone controls intercourse with foreign states and its citizens should have only its money and never that of other countries. So long as the state is anything other than the spontaneous organization of the people, the latter are not free. An absolutely free people would need no state. The fate of the true culture of freedom rests with regenerated Germany. Her language which has been developed indigenously, without obscured etymologies, from a primitive kindred people and not adopted or borrowed, or adumbrated by change, like that of other European nations, makes true mental freedom possibly only for her people. His philosophy and his political theories, it is concluded, are both superceded by later and better views, but will yet long remain, even where most contradicted, very instructive and elevating.

In Wolff's expulsion from Halle, Zeller sees the pure epitome of a contest which is not yet ended. At the close of the thirty years' war Germany had grown barbarous, ignorant, schismatic, sensuous in taste and life to a degree which German patriotism now finds it hard to admit. Protestantism had fallen into the hands of men whose rule was scarcely less fruitless, formal and unprogressive than that of the Jesuits then dominant in the catholic church. It had no understanding of the religious needs of the people and had driven edification from the church, learning from the schools, and freedom and thoroughness from the universities and from literature. At this period pietism and philosophy first took their rise in Germany. Spencer, reacting against the dry and dead intellectualism of theology, urged at first a most salutary form of emotional and practical religious living, and argued the necessity of a definite and typical change of inner life which found wide acceptance

and has founded the Lutheran church deep in the *Gemüth* and given it its peculiar freedom and independence of scientific reason. Wolff, whose methods affected mathematical form and certainty, who used the vernacular tongue in his thronged philosophical lecture room, who had argued that even an atheist might lead a moral life, and that if no divine reveletion had been made even reason would incline men to virtuous lives, was violently attacked by the pietists and obliged to enter into tedious and profitless disputations. He saw his students and followers and even his friends gradually alienated from him until at length the king of Prussia, induced by a plump lie of an enemy of Wolff, ordered him to leave his domains within forty-eight hours on penalty of being hung, and made it a crime to circulate or read his writings. This his pietistic colleagues declared was in answer to their prayers. He was recalled late but not until his vigor and his influence were forever impaired.

The relations of church and state in the past and present and the discipline, cultus, orders, property and influence on education of the former are discussed from an abstract, moral stand-point in a readable little volume, which space fails us to epitomize. (1). In another essay

(1). Staat ñ Kirche: Vorlesungen an der Universität zu Berlin, Gehalten von Edward Zeller, 1873, p. 250.

the trial of Galileo studied in part from original sources, is described, Schwegler, Waitz and Lessing as theologians are characterized, the relations of policy and justice, and of nationality and humanity are discussed, and the present condition and problems of German philosophy, and of the *theory of knowledge* (Erkentniss-theorie) are explained.

The latter has been the central question in German philosophy since Kant brought into flux the question of the origin and truth of our notions of things. Based upon special solutions of it, the great idealistic systems were wrought out. The cry "back to Kant," and the general abandonment of the foundations upon which Fichte, Schelling and Hegel built, which in many quarters has degenerated to an uncritical cultus of Kantain orthodoxy was at first matured by the conviction that, he alone had fairly examined and justly estimated the importance of the "theory of knowledge," while later the experimental psychology of the physiologists and the studies of Helmholtz have only more specially elaborated this theory and more critically answered Kant's problem. We must not infer from the study of Kant that experience can give *unordered* matter, or that all form is innate, still less can we agree with him that because we apprehend things by means of subjective forms we must necessarily be ignorant of things as they are in themselves. There is another case. Perhaps the forms are adapted by nature to give us the *right* view of things. Subjective and objective belong to one nature. True if we isolate one phenomenon we cannot distinguish its elements, but every new observation applies the method of difference. We prefer to say that with the increasing compass and accuracy of our knowledge, it approaches practically—though not theoretically in the sense of Fichte and Hegel—to absoluteness. It necessarily grows certain as it grows wide. It is reflection which sifts out the *a priori* elements from experience and thus brings knowledge of things. Hence logic is grounded on the theory of knowledge, which must in turn be completed by it. Number, time and space Zeller makes the most general forms of connecting objects. Properties are causal ideas which are not innate in the old historic sense—so intimately connected with the doctrine of pre-existence—but they are hypotheses to explain the unifying impulse of the mind. Space, however, unlike the other two which are objectively real, at the basis of being and change, may be only the general way in which things impress us, or a general form of reaction of our organism in its habit of connecting sensations. Different hypotheses of the external

cause of sensation may supersede it; or again tri-dimensional space may be only a special case of another relation, embracing other cases also.

Here at last we glimpse a limit to Zeller's remarkably wide critical horizon which is particularly manifest throughout his courses of psychological lectures. The hypothesis of a fourth dimension of space, in no way destroys the validity of the old geometrical three. Mathematical physics has elaborated equations containing functions which might be true in a space of x dimensions and forthwith metaphysical psychologists, reasserting old idealistic traditions, or perhaps too easily bullied by scientific authorities, stultify science by talking of an absolutely spaceless universe, and a non-extended matter. To Zeller this is only a logical possibility which must not be forgotten. Like many of the older German professors of philosophy he does not deem it all too unworthy the dignity of his department to interest a curious class by recounting some of the more striking results and methods of the physiological study of sensation, but too often only to disparage their philosophical importance and to limit to the narrowest the impressions deduced from them in detail while roundly acknowledging the general importance of such investigations for the theory of knowledge. The fundamental importance and the immense scope of these, centering as they do about the transforming psychological conception of reflex action modified by specific nervous functions and inhibition, Zeller fails adequately to appreciate. We will pause here only to observe that the whole drift of German physiology is now strongly and almost without exception against the possibility of such materialistic deductions as Zeller fears therefrom.

Philosophical truths to Zeller are not coins stamped and weighed to pass unchanged from hand to hand, but historical products deeply rooted in personal, national and religious character. As such they must first be approached and studied if we would add our own individual thinking as a contribution however trivial to the thoughts of the race, instead of reviving old issues, resolving old problems and thrice slaying the slain. The history of philosophy is thus a labor-saving department of study most economic of mental effort, and prepares men for the problems of to-day. It should aim in the first place simply to present and not to criticise or estimate its subject matter. It should teach us how our consciousness became what it is. It should show that all practical sciences or institutions of human life and society pre-suppose a theoretical foundation which is deep and broad, in proportion as they are high or important. Not only do the roots of all things go back to philosophy but it is an unnatural condition of things if philosophy is suspected or degraded. As Cavour said the state should be occasionally led back to first principles, even by revolution if need be, so it is we not to allow men entirely to forget how law and every political and social institution were at first and still are at bottom, only devices to establish simple morality as an individual habit, and between man and man, and that all religions are but formulations of man's relations to the universe as a whole. Moreover the special branches of knowledge are able to act and react fruitfully upon each other only as it is seen how organically they are connected. The effect of science upon philosophy may be in some sense compared to its effect upon poetry. Since it became impossible to believe longer in myths, poetry, instead of being crippled or suffering any limitation of her domain as many predicted, has found new sources of inspiration deeper and stronger than ever before while even historic myths exert undiminished magic charm over the imagination of men. So likewise the metaphysical myths, Platonic ideas and ideals, innate intuitions, an absolute ego, a dialectic, world-developing reason, a universal will, and scores more, are no less quick-

ening now than before while the observational and more exact experimental study of the psychic powers are opening up a radically new conception of the human soul, reason and conscience. With this is suggested, at least to those where supreme passion of life it is to conceive it however faintly, the possibility again of one organized intellectual world manifestly monistic, without unscientific, hyper-logical guess-work, in which idealism and realism, instead of being absolute even in their opposition are simply two cardinal points of direction of which philosophic thought must not lose sight.

In his somewhat popular history of German philosophy since Leibnitz [1] written as the thirteenth volume of the History of Science in Germany under the auspices of the Saxon commission, somewhat monographic, and mainly devoted to the seven great names from Leibnitz to Schopenhauer both inclusive and not to be compared with Kuno Fischer's exhaustive work in the history of modern philosophy, Zeller urges that the reformation made Germany introspective. The deepest roots of her power in the world's history he finds in her philosophy and more especially in her idealism at once its weakness and its strength. Germany will be false to all her traditions if she forgets the power of subjective reflection. Her philososhy was developed in a period of peace unbroken save by the inspiring war of liberation, and even now with all her political military and material successes, her growing love of money, and devotion to business must be guarded with pious patriotic care as yet full of saving and guiding power.

Zeller's great life-work is of course his history of Greek philosophy, the first part of the first edition of which was printed more than a quarter of a century ago and which has now reached a fourth edition. It is by far the best work on the subject. His characterization of the pre-Socratic philosophy, though as unlike the speculative histories of Hegel or Schwegler as possible, is a masterpiece of constructive criticism. The laborious minuteness with which every trace of suggestion is followed up, the compass of his method which requires familiarity with every phase of contempory Greek life and history, the conscientious care to avoid all false idealizations and to hold every personal preference or prejudice in perfect poise and his constant verification by quotations have all combined to make his readers conceive of Greek thought as perhaps less pure and perfect and less transcendently wonderful than we were wont, but have invested the theme with a nearer and far more sympathetic human interest than ever before. It is of course impossible in our limits to enter into any detailed review of this work, but this rough sketch of its author's varied intellectual labors will not have been written in vain if it shall induce the reader to take this work seriously in hand for himself.

ED.

[1] Geschichte der Deutschen Philosophie seit Leibnitz von E. Zeller, 1873, pp. 917.

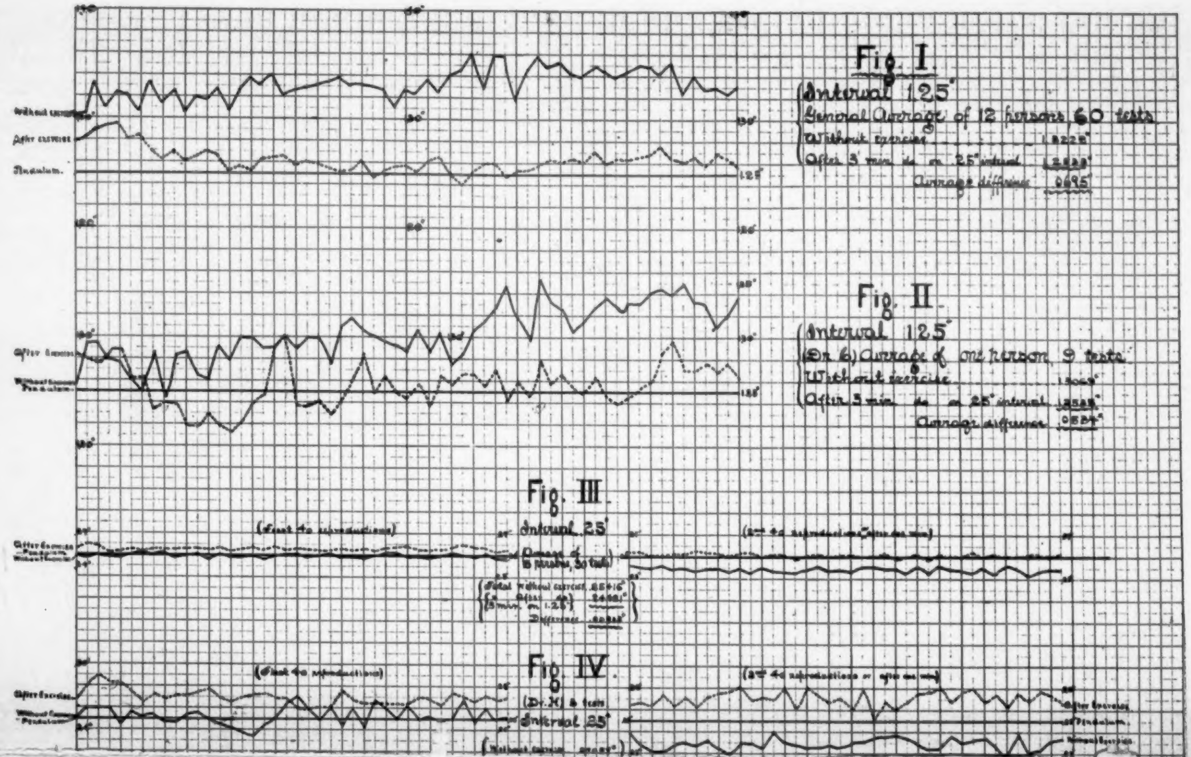


Fig. I.
Interval 1.25"
General Average of 12 persons 60 tests
Without exercise 1.2228"
After 3 min. do. on .25" interval 1.2533"
Average difference .0695"
Without exercise
After exercise
Pendulum
Fig. II.
Interval 1.25"
(Dr. 6.) Average of one person 9 tests
Without exercise 1.3009"
After 3 min. do. on .25" interval 1.2525"
Average difference .0584"
After exercise
Without exercise
Pendulum
Fig. III.
Interval .25"
(First 40 reproductions)
Average of 6 persons, 30 tests
Difference
Fig. IV.
(Dr. H.) 6 tests
Interval .25"
(First 40 reproductions)
After exercise
Without exercise
Pendulum

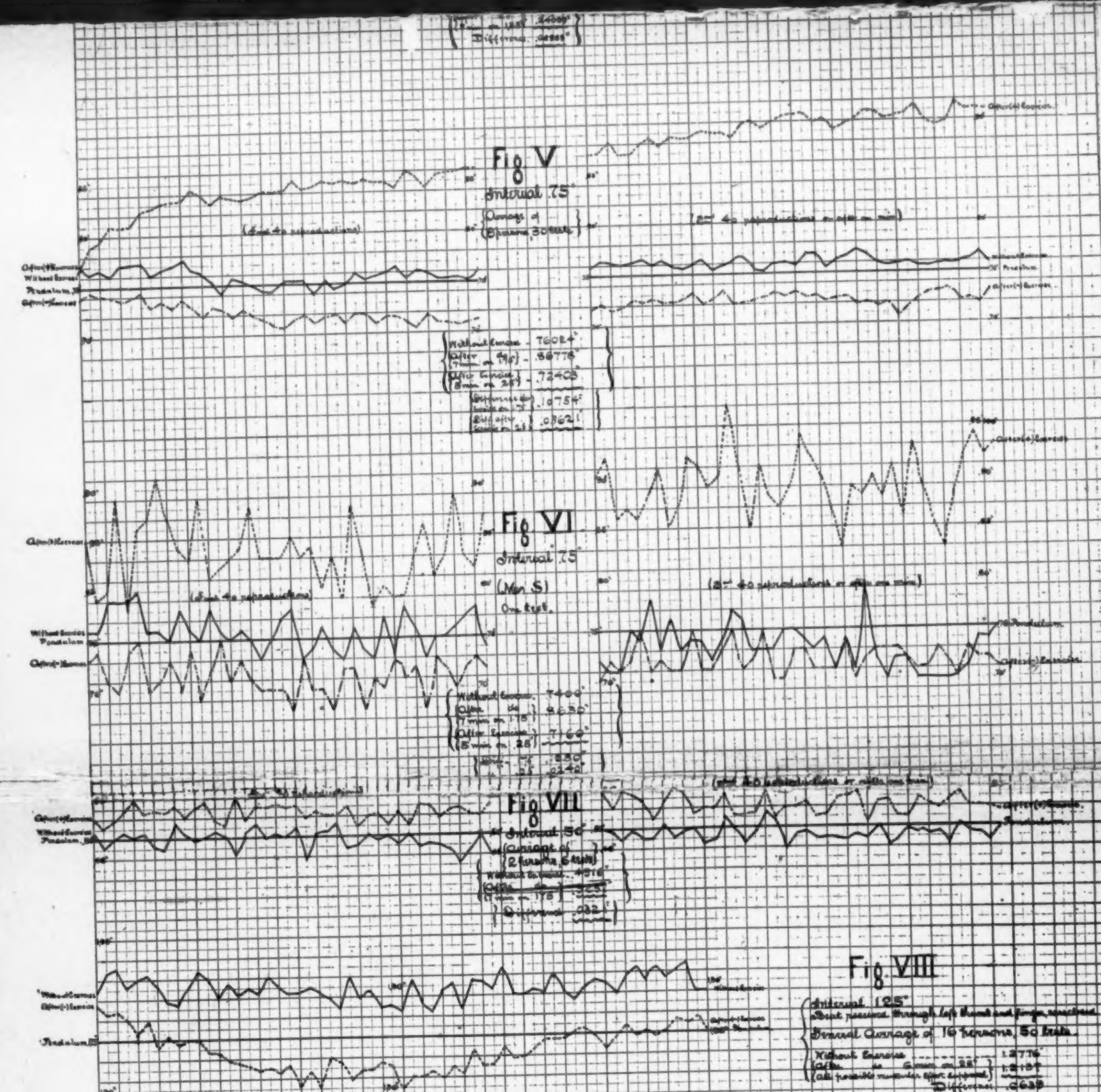
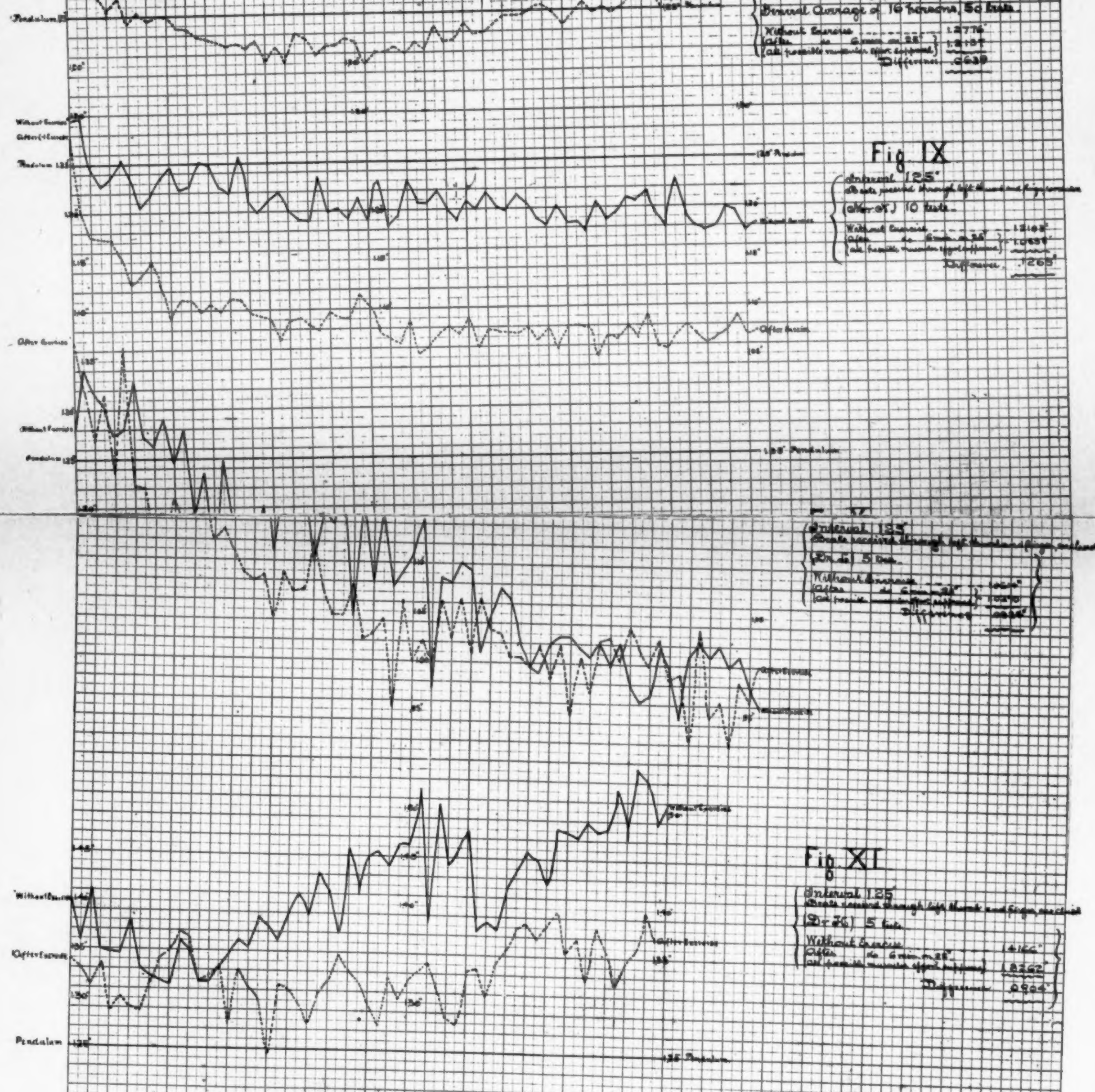


Fig. V
Interval .75"
Average of 9 persons, 30 tests
Without Exercise .76024"
After .88776"
After Exercise .72403"
.10754"
.08621"
Fig. VI
Interval .75"
(Mr. S)
One test.
Without Exercise .7400"
After .9630"
After Exercise .7160"
Fig. VII
Interval .50"
Average of 2 persons, 6 tests
Difference .032"
Fig. VIII
Interval 1.25"
General Average of 16 persons, 50 tests.
Without Exercise 1.2776"
1.2137"
Difference .0639


General Average of 16 persons, 50 tests.
Difference .0639
Fig. IX
Interval 125°
10 tests.
Difference .1263
Interval 125°
5 tests.
Fig. XI
Interval 125
5 tests.
Without Exercise
Difference .0904
Pendulum
125° Pendulum